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☞ An asterisk (*) denotes an illustrated article

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WISHING TO MAKE THIS JOURNAL an organ of intelligent discussion to those of our readers who may wish to gain or give information on the subjects to which its columns are devoted, the publishers solicit letters from all among them who make the study of these subjects a pleasure, or a profession.

Subscribers would confer a favor upon us by remitting *CHECKS*, or *POST OFFICE MONEY ORDERS*, as we are frequent losers where money is enclosed in letters.

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EXTRA COPIES OF THE JOURNAL.

Those of our readers who desire extra copies of the numbers of the JOURNAL, containing the proceedings of the Cincinnati Meeting, will confer a favor by forwarding their orders, stating number of copies required, at once.—ED.

RESIGNATION OF CAPT. W. H. WHITE FROM THE CITIZENS COMPANY, BROOKLYN.

The retirement of Captain Wm. Henry White, from the position of Engineer of the Citizens Company of Brooklyn, during the past month, was made the occasion of a very kindly expression of feeling on the part of the men in his employ, in the form of resolutions, and the presentation of a very handsome ebony stick, gold mounted, etc. The Board of Directors also passed a set of resolutions, filled with commendation for past services, and the expression of most kindly feelings, and good wishes for the Captain's success in his new business.

It will be a pleasure to many of our readers to know that, in leaving the manufacture of gas, the Captain is not to be lost to the profession; but is to be found at the other end of the line, at the point of delivery to the consumer; and it is to be hoped that the result will be that there can be no more complaint on account of the meter.

Cantor Lectures.

The Chemistry of Gas Manufacture.

By A. VERNON HARCOURT, Esq., M.A., F.R.S.
Lee's Reader in Chemistry at Christ Church, Oxford, and one of the Metropolitan Gas Referees.

[From the London Journal of the Society of Arts.

LECTURE III.—MONDAY, MARCH 19.

I may illustrate this point in another way. I have here two exactly similar Argand burners, which I will adjust so that their flames may be equal, and now I am going to substitute for the chimney of one of them a much longer chimney. You will see, on comparing these two flames, that the difference between them is something like the difference between the appearance of the jet of coal gas burning in the bottle of oxygen, and that of the same jet burning in the air. The burner with the longer chimney has a whiter flame, and, considering only the unit of area, a brighter flame, but on the whole the photometer would show that is giving less light than the other. For we must remember that the total luminosity of a flame is the product of two factors—of the brightness of the unit of area and of the total number of such units. In other words, flames differ one from another both in brilliancy and in size, and the quantity of light emitted by a flame depends upon its brilliancy and also upon its size. In this case, the effect of putting on the longer chimney is that there is a much stronger draught and more air is drawn through than is needed for the most advantageous combustion of the coal gas; and consequently although the actual flame is brighter, the total amount of light got out of the same volume of gas is less than it is in the other case.

I have here a drawing of one of these Argand burners, made to show you some of the points of importance in their construction. One I have already called attention to—namely, the proportion between the quantity of gas that is burnt and the size of the chimney. It is of great importance that the chimney, and also the openings for the admission of air below, should be of the right dimensions for the quantity and quality of gas that is burnt; and these are matters of very fine adjustment, so that one ought properly to have a different chimney for the same rate of burning for each quality of gas, or for the same quality of gas burning at different rates. The manner in which the air reaches the gas is also of importance. The sectional drawing shows a cone, the object of which is to cause the current of air to impinge on the flame of the gas. In an Argand burner there is an admission of air through the centre, and so the flame burns in a ring, the air having access to it both within and without. Another essential point which the experiment I have just shown illustrates is, that the gas should issue into the air very gently. If it enters with a rush, it mixes itself with the air, and there is a loss of light. Consequently, in well made burners (Argand) the holes in the steatite chamber, from which the gas burns, are made very large relatively to the quantity of gas which is to pass through them.

I pass now to the measurement of the illuminating power of gas, and I will illustrate to you the manner in which the estimation of the illuminating power of gas is commonly made, by taking this candle as the unit of comparison, and this burner, which is one of the standard burners that are employed in photometry, for the burner at which the gas is consumed. I have here also one of the discs which are used in photometry. It consists of three paper discs of the same size, the outer ones being of thin paper, and the inner

one of stouter paper, with a star cut in the centre. Either such a disc as this, or one with a round grease spot in the centre, will serve for the purpose.

If I place this disc midway between the candle and the lamp, it receives the light of the candle on one side and the light of the lamp on the other, and since the light of the gas lamp is very much greater than the light of the candle, you see on the side on which the candle is, a dark star on a white ground. But if I bring the disc near to the candle until the light which reaches it from the candle is greater than that which reaches it from the lamp, then the conditions are reversed, and you see a bright star upon a dark ground. If I move it between the two until the light which reaches each side of it is equal, the star disappears. Therefore, the disappearance of the star serves as an indication that the disc is so placed between the candle and the gas flame as to receive equal light from each. Now we know that the light which comes from any source of light varies inversely with the square of the distance of the object illuminated from that source. We, therefore, can compare the amount of light proceeding from the gas flame and from the candle, by simply measuring the distances between each of them and the disc. If I were to measure the distances between the candle and the disc, and the lamp and the disc, when the disc is in position of equal illumination, I should find probably that the distances were very nearly as four to one—the distance from the candle to the disc being about one quarter of the distance between the disc and the gas lamp. This would be the case if my standard candle were consuming sperm at the proper rate, and if the gas were burning at the standard rate of five feet an hour, and had the illuminating power of 16 candles. Or, to argue from the observed intervals to the illuminating power; since the amount of light varies inversely with the square of the distance from the source of light, and the ratio of the two intervals is as four to one, the illuminating power of the gas, compared with the illuminating power of the candle, is as sixteen to one. It is in this way that the luminous power of a sample of gas is estimated.

It is, however, necessary that the gas should be burnt under certain fixed conditions. The rate at which the gas is to be burnt has been fixed at five cubic feet an hour, and the burner at which the gas is to be burnt has also been fixed. During the past year the standard burner has been defined by an exact measurement of all its principal parts, the height of the cone and of the steatite chamber above the gallery on which the chimney stands, the size of the holes from which the gas issues, and the internal and external diameter of the steatite chamber. This burner was constructed by Mr. Sugg, and is a most excellent burner, although I believe he has improved upon it since. The fixing of the burner by Act of Parliament was a very important step, and one quite in the right direction, in order to establish a fixed measure of the illuminating power of the gas which the gas companies undertake to produce.

Besides a fixed manner of burning the gas, it is also necessary that there should be a standard of illuminating power; and that which is used in this country for the standard is a sperm candle, one of six to the pound, and burning 120 grains an hour. The ordinary way in which the testings are made is by using two such candles fixed side by side, and comparing their light, by means of the disc in the manner I have described, with that yielded by the gas burning at the rate of five cubic feet an hour to the standard burner. There are certain corrections which need to be made in using the candles. Although they are intended to burn at the rate of 120 grains per hour, sometimes; as one would expect, they burn more, and sometimes less, than that amount. It is assumed (although the assumption is only an approximation to the truth, that the light given by the candles will vary directly with the weight of the sperm consumed; and, accordingly, the candles employed are weighed before and after each testing, and a proportional correction of the result is made, if more or less than the proper quantity of sperm has been consumed. Candles are certainly a very imperfect standard of comparison. The amount of light that the candle gives varies, not only with the rate at which the grease is being consumed, but with a number of other conditions, with the curvature of the wick, with the surrounding temperature, and with the way in which the candle is burning from minute to minute, a different amount of light being produced by a candle when the cup is full of grease, and when it has been drained dry by guttering or the capillarity of the wick. These differences are by no means inconsiderable, and, consequently, photometric observations are always uncertain, however cerefully they have been made, since the standard used for comparison is itself variable. I have been endeavoring for some time to obtain a substitute which should be more satisfactory than the candles, and I wish, in concluding this lecture, to bring before you a form of standard light which I have recently devised.

What are the points in a candle which are unsatisfactory, and in what is a change needed in order to obtain a more satisfactory method? First there is the uncertainty as to the composition of the candles. I believe that they are not made of pure sperm, which is too brittle, and that the manufacturer needs to mix from 5 to 10 per cent. of white wax, or a small quantity of paraffine, with the sperm, to make a serviceable candle. Next, the number of strands in the wick, which is the burner of the candle, may be varied, and the size and formation of the wick have a considerable effect upon the illuminating power. Also, as has been pointed out, the mode of burning and light of the same candle fluctuates greatly from time to time.

In order to have a material which should be more definite, I propose to employ an "air-gas," a mixture of light petroleum with air. Such a gas may be readily obtained of any desired illuminating power. The light petroleum may be procured in large quantities at a moderate price, and by three or four rectifications between 50° and 60° C., a product is obtained which volatilzes into the air readily and wholly at a low temperature. The gas which I am here burning was made by passing air through the water which is contained in this Woulfe's bottle, having previously placed upon the surface of the water a measure of light petroleum. The whole operation of making the gas consists in charging a pipette with light petroleum, placing the point of the pipette within the Woulfe's bottle, and allowing the volatile liquid to flow down the surface of the water, quickly replacing the corks and connecting the outlet tube of the bottle with a gasholder, increasing the weight of the counterpoise and opening the tap. The air then bubbles up through the water, and through the layer of petroleum which floats upon the water; the petroleum volatilizes, and mixes with the air. In order that the gas may have a definite illuminating power, it is necessary to take known amounts of air and petroleum, and such a quantity of air and petroleum so volatile and in such quantity, that the whole of the petroleum employed may volatilize into the air. The reason for having water in the bottle is, that for the volatilization to proceed regularly, there should be a level surface, which the water provides, and also that a quantity of heat should be supplied to the petroleum, for it becomes very cold by its own evaporation, and that heat the water furnishes.

Next, it is requisite to have a very definite burner. That it should be a good burner is not at all necessary; but it is essential that it should be thoroughly definable. The burner I have here is a very simple one; it is a brass tube, four inches long and one inch in diameter, the upper part being closed by a plug, half an inch in thickness, with a cylindrical hole in it, the diameter of which is a quarter of an inch. The gas therefore issues practically at no pressure at all and passes freely out into the air. A pair of such burners are fixed side by side, like the candles in the holder of the photometer. In each tube I have packed a little asbestos, in order to have the means of equalising the proportion in which the gas divides itself between the two. That, however, has no influence at all upon the way in which the gas-flame burns; the upper part is clear, and the gas streams out freely into the air.

The first step towards obtaining a standard gas was to ascertain experimentally, with various proportions of air and petroleum, which proportion gave with this burner the most constant flame, and at what rate each sample needed to burn to give from each gas burner the light of one candle. I find I can make the gas such that the rate of burning is a convenient round number. The gas is now passing through the meter at the rate of one cubic foot an hour; and the two flames are now giving, but much more regularly, the same light as the standard candles burning beside them.

I have made many experiments, comparing in a photometer the light of such gas-flames with the light of the candles, and altering slightly the proportion of petroleum until I have got this equality. I think that this gas-flame will have, as a unit of light, the advantage over the candles of greater definiteness of substance. and far greater definiteness in the conditions of burning. I find also that the flame is very much steadier in the photometer than the candle flame. The whole experiment lies in the hands of the operator; he has not to depend on the maker of the candles, for he makes the standard of the combustible himself. The burner is perfectly definite, and the gas itself is very readily and quickly prepared. The rectification of the petroleum is an easy operation on a scale which yields enough petroleum for the preparation of from 10 to 20 samples of standard gas.

The question of the unit to be employed in the measurement of the illuminating power of coal gas, which I have thus attempted to solve, is the last topic on my list, and concludes the course of Lectures which your Council have done me the honor to ask me to undertake.

Importing Gas Coals from England.—Boston is importing her gas coal from England, for the first time in five years, refusing to submit to the bituminous monopoly. A Red Star steamer has arrived with sixteen hundred tons.—*Ibid.*

The Consumers' Gas Co., Baltimore.—This company expect to furnish gas in about two weeks. They have fifty-four miles of pipe down, and are now putting up the spans on Monument and Crosby streets to carry the mains across Janes' Falls.—*Ibid.*

Fifth Annual Meeting of the American Gas-Light Association.

HELD IN CINCINNATI, OHIO, OCT. 17, 1877.

OFFICIAL REPORT.

[Continued from page 188.]

AFTERNOON SESSION.

The Convention assembled at 3 P. M., and were called to order by the President, who said: I am sorry the members of the Association are not more prompt. Our time is precious and we can't afford to waste it. We have five papers here to read, and it will require considerable time to dispose of them all. I wish to inform the members that the officers of the Mercantile Library Association have extended an invitation to the members of the Association to visit their rooms. They have papers there from all parts of the country, which you can see; and you can also look at the books in the library, by simply stating that you are members of the Gas Association. The rooms of this association are in this building just at the head of the stairs.

I also want to request any member who was not present when the roll was called, to hand his name to the Secretary, so that it may be noted by him.

I have here a letter from Mr. Price, our First Vice-President. I understood, from the telegram he sent, that there was sickness in his family; but it appears from the letter that that was a mistake.

The letter is as follows:

OFFICE OF CLEVELAND GAS-LIGHT AND COKE CO.,
CLEVELAND, OHIO, Oct. 16, 1877.

Chas. Nettleton, Esq., Secy., Cincinnati, Ohio.

My Dear Sir, Until this morning I fully expected to attend the meeting of the Gas-Light Association. The man upon whom I rely to care for matters in my absence, was taken very ill last night, and I cannot well leave. I have builders at work, and it is difficult to get away on that account; but I should not allow any ordinary difficulty to prevent my visiting Cincinnati at this time. My interest in the American Gas-Light Association is undiminished. I have been absent from but one meeting since its organization, and then I was out of the country.

Please make my apology to the Association, and to any inquiring friends convey my kindest regards.

Believe me, dear sir, very truly, yours.

W. H. PRICE.

Now, gentlemen, I have on the table here several papers to be read, and I don't suppose it matters in what order we take them up. When we took our recess, the subject under discussion was the paper of Mr. Forstall; and if any member desires to speak any further on that subject, or wants to ask any questions of Mr. Forstall, or any other member of the Association, with regard to the subject of his paper, they will now have the opportunity.

Mr. Henry Cartwright—I would like to know the size of Mr. Forstall's stand-pipes.

Mr. Forstall—Four inches in diameter for the whole time included in this paper. In the new retort house we have six inch pipes, but I don't find any difference between them. I don't think the four inch pipes stopped up any oftener, or any worse than the six inch pipes do now—in fact, if there is any difference, I think the six inch pipes stopped more frequently than the four inch did. Our old works were built in England, and put up on their plan, and it is not their custom to use as large stand-pipes as we do in this country.

Mr. Henry Cartwright—The custom here seems to be drifting towards the enlargement of stand pipes. Four inches used to be the maximum, but we see a great many as high as eight inches.

Mr. McElroy—Our stand pipes are six inches in the new works, and the dip-pipe eight.

Mr. Henry Cartwright—Mr. Forstall; please give us some information as to the exhauster. What exhauster do you use?

Mr. Forstall—We used the MacKenzie exhauster. We occasionally used Beal's exhauster, but there was no apparent difference in the results. With the steam jet we got a steadier pressure, but the use of different exhausters did not affect the pressure on the hydraulic main.

Mr. Elroy—What is the shape of your retorts?

Mr. Forstall—They are "D" retorts, sir, with the bottom a little oval—very little—the corners just rounded off. The radius of curvature of the bottom is about three feet, and I suppose they can fairly be called "D" retorts. I found in the old retorts that there was a tendency for carbon to gather in the sharp corner, at the base of the "D," and I cannot say that we have been entirely successful in removing that difficulty, for I find that we gather about the same amount of carbon proportionally, and there is the same stoppage in the stand-pipes. The pipes of the upper retorts stop oftener than the others—twice as often. To avoid that as much as possible our front walls are thirteen inches thick. We tried protecting the pipes by a loose ticket of sheet iron; but while we had these shields on the stoppage went further up the pipes than before, but did not diminish in frequency or amount.

Mr. Littlehales—I found an increase in the amount of carbon deposited, after the retorts have been once let down and heated again. It forms a surface upon which the carbon can collect, and it collects more frequently after the retorts have been let down once or twice, than in new retorts.

Mr. Forstall—Mr. McElroy, in your new house are the stand-pipes any shorter than before?

Mr. McElroy—No, sir; they are about the same length.

Mr. Forstall—I have found that the length of the ascension-pipe has a great deal to do with its stoppage. The shorter the pipes the oftener they are obstructed.

Mr. McElroy—In our establishment the stand pipes of the lower retorts stop much oftener than the middle pipes. Our center retorts are worse than either the upper or lower.

Mr. Crenshaw—I hope Mr. Forstall will describe his method of removing the seal.

Mr. Forstall—That is no more than a patent process, one out of fifty, which I have no business to puff here any more than any other.

Mr. Burtis—As this seems to be a sort of experience meeting, I will state what our experience has been in the use of different-shaped retorts. At the north side station we have "D" retorts, and at the south side station we have ovals, and we find no difference whatever, so far as the stoppage of the pipes is concerned, between the oval and the "D" retorts. If there is any difference there might be a little more carbon formed in the oval than in the "D" retorts. The only reason for that, I think, is that when we have a seal on them it is a little deeper than the seal used at the other station.

Mr. McIlhenny—I will ask Mr. Forstall how often it is necessary to clean the carbon out of the retorts?

Mr. Forstall—On an average, once in three months. We generally do that on Sunday now, because we suspend the manufacture of gas on that day.

Mr. McElroy—How thick is the carbon before you clean?

Mr. Forstall—It varies; but we generally clean the retorts when their capacity is reduced by the deposit below our minimum charge of 200 lbs. of coal.

Mr. McIlhenny—Before unsealing your dips how often did you clean them out?

Mr. Fortsall—Once in six weeks. Under the old system the carbon was deposited more irregularly than it is now, so that, although the total quantity in the retorts might not have been greater than it is now when cleaned out, yet we had to clean oftener to get the lumps out.

Mr. Somerville—I think the coal has something to do with the amount of carbon deposited. I generally have just enough carbon formed to keep my retorts gas-tight; but last summer there was a strike in the coal mines where I get my coal, rendering it necessary for me to change, and immediately there was an excessive formation of carbon in the retorts.

Mr. McMillan—Mr. Forstall accounts for the increase in his yields, during the second period, by saying that the furnace gases were drawn into the retorts, and his statement seems to be very generally accepted; but I don't see how any part of it was due to the air drawn in by the exhauster. I think there is oftener a pressure than a vacuum, and I don't understand why he would not lose as much as he would gain—perhaps a little more. I don't know that I have ever thought of it before, but it occurs to me today that in breaking up the molecules of hydrocarbon, the quantity of gas is increased by the formation of free hydrogen, and the loss or gain is in that way and not by the air.

If you have an average pressure of 2·7 inches, and an inch vacuum, I don't know how it would be possible to gain very much by the air drawn in.

Mr. Forstall—I didn't intend to lay much stress upon the gain in the retorts, but I would like to have the gentleman account for the immense deterioration in quality, for we had twenty per cent. less illuminating power, with a greater percentage of gas.

Mr. McMillan—I think the deterioration is attributable to the fact that you drew in oxygen and let out gas about equally, and when you decompose your hydrocarbons, and form free hydrogen, you would deteriorate the illuminating power of the gas very rapidly. I know air does go in, and I think just as much gas goes out.

Mr. Forstall—There was carbon on the inside which would prevent leakage outwards to a certain extent; but the oxygen might come in through the pores of the retort and be there consumed, forming carbonic oxide, without much loss of coal gas from the reaction of the pulsations. In our case we found a loss of twenty-two per cent. Carbon was formed, and, of course, I don't pretend that there was much gain, but it was sufficient to produce carbonic oxide and deteriorate our coal gas.

Mr. McMillan—I can't yet understand how it would be possible for the oxygen to get in where the gas could not get out.

Mr. Forstall—I didn't pretend to say that no gas leaked out. My intention was simply to show the loss of illuminating power due to that mode of working the exhauster. I suppose gas may have gone out too, during the pulsations, but it was not as pure hydrogen, for there was no pure hydrogen in the retort.

Mr. McMillan—We don't know that there was no pure hydrogen there.

Mr. Forstall—Well, that is a matter for future experiment of course.

The Secretary—I want to ask you, Mr. Forstall, whether, when you ran under pressure, the carbon was harder than when you were running under no pressure?

Mr. Forstall—I could not tell you, really, for my attention was not called to that point at all; but the carbon formed at present in our new retort house appears to be softer than in the old works.

Mr. Helme—It seems to me it would be impossible for something to pass in while something was passing out all the time. There must be pressure from the outside, if there is a vacuum, that would keep the hydrogen from coming out.

Mr. McMillan—He, at one time, has an inch and a half vacuum, and, at others, four inch pressure. It is when the pressure is in the retort that the gas leaks out.

Mr. Gray—What about the comparative time for cleaning the retorts under pressure and without pressure, and also the comparative time of cleaning the stand pipes with and without pressure?

Mr. Forstall—The first portion of your question has been answered—3 months with dips, and six months without; the second part of the question I can answer at the next meeting of this Association; but, at present, I am not prepared to do it. I am keeping a daily record of everything connected with this subject, the results of which I will endeavor to present at the next meeting.

Mr. Battin—Was there an analysis of the gas made at different times?

Mr. Forstall—No; not an exhaustive analysis; but the specific gravity was taken, and found to be very heavy—more than our average.

Mr. Battin—I suppose then you were perfectly satisfied that the gas was heavier?

Mr. Forstall—Yes, sir; I had no doubt about it.

The President here stated that about as much time had been consumed by the discussion of Mr. Forstall's paper as it was possible for the Convencion to bestow upon it; and he, therefore, announced the next paper, which was read by Mr. Rankin, on "Carbonizing Coal."

Before commencing to read, Mr. Rankin said:

I want to request the Association to make as little disturbance as possible, because my voice is not very strong, and the less confusion there is the more apt you are to hear. [A voice—put on more pressure.] I will state that we always work under a low pressure.

Mr. Rankin's paper, on

Carbonizing Coal.

It has been said, that "Science and Art differ in this, that Art says—give us Rules; while Science demands Reasons." If so, ours then may be deemed a profession, as we require both rules and reasons. But are we not too often content to work by rule, rather than seek for and demand substantial reasons.

The subject that I have chosen for this paper, viz, carbonizing coal, lies at the very beginning, in fact, out of it spring all others connected with our calling. Much profound thought, and patient investigation, has been bestowed on this subject by those who were far more competent than the writer, therefore, I could not expect to say much that has not already been said; but feeling that there is room for improvement both in the apparatus and process as usually employed, and with a desire and hope that the discussion of the subject may suggest something practical, and a decided step in advance. I would, with becoming modesty ask your attention for a short time.

In the manufacture of illuminating gas the proper means of carbonizing coal is of the first importance, ignorance on this point has often entailed very serious loss, when by a little intelligence, satisfactory Profits would have resulted.

[Continued on third column, next page.]

Official Report of Examinations of Illuminating Gas, for the week ending October 6, 1877, made at the Photometrical Room of the Department of Public Works.

Date.	Time.	Temperature of Thermometer.	Barometer.	Gas Company	Burner.	Pressure at Point of Ignition.	Consumption of Gas. Rate per hour.	Consumption of Candle, Grs. per hour	Illuminating Power. Observed.	Illuminating Power. Corrected.
1877.	-	Deg.				In.				
Oct. 1	9 A. M.	72	30.255	New York	Sugg-Letheby	.05	5.01	123.0	16.96	17.53
" 2	9.30 A. M.	72.5	30.200	"	"	.05	4.96	121.2	16.48	16.77
" 3	9 A.M.	72.5	20.125	"	"	.05	5.00	121.8	16.60	16.85
" 4	10.30 A.M.	74	29.818	"	"	.05	5.08	118.2	17.52	16.97
" 5	1.30 P.M.	69	29.826	"	"	.05	5.01	123.0	15.92	16.28
" 6	12 M.	66	30.267	"	"	.05	4.96	120.0	16.84	16.97
									Av'ge.	16.86
" 1	8.30 A. M.	72	30.255	Manhattan	"	.05	4.96	124.2	17.00	17.73
" 2	9 A M.	72	30.200	"	"	.05	5.03	123.0	18.04	18.36
" 3	9.30 A.M.	73	30.125	"	"	.05	5.08	118.2	18.40	17.83
" 4	10 A.M.	74	29.818	"	"	.05	5.00	123.6	17.60	18.12
" 5	2 P. M.	69	29.826	"	"	.05	5.04	121.5	17.16	17.22
" 6	11 A.M.	65.5	30.267	"	"	.05	5.00	121.8	17.72	17.98
									Av'ge.	17.87
" 1	9.30 A.M.	72.5	30.255	N. Y. Mutual	"	.05	4.05	120.0	16.54	20.41
" 2	8.30 A.M.	72	30.200	"	"	.05	4.12	124.2	15.44	19.34
" 3	10 A. M.	73	30.125	"	"	.05	4.22	120.0	17.40	20.60
" 4	9.30 A. M.	73	29.818	"	"	.05	4.08	120.6	15.36	18.90
" 5	2.30 P. M.	69.5	29.826	"	"	.05	4.71	123.0	15.96	17.36
" 6	10.30 A.M.	65	30.267	"	"	.05	4.64	118.8	17.48	18.63
									Av'ge.	19.20

For the Week Ending October 13, 1877,

Oct. 8	1.30 P.M.	63	30.303	New York	Sugg-Letheby	.05	4.90	121.8	16.28	16.85
" 9	12 M.	66	30.074	"	"	.05	5.03	124.2	16.84	17.31
" 10	9 A. M.	66	30.192	"	"	.05	5.10	123.0	17.08	17.16
" 11	10.30 A.M.	67	30.003	"	"	.05	5.07	123.6	16.88	17.13
" 12	10.30 A.M.	65	29.992	"	"	.05	5.08	120.0	17.00	16.73
" 13	10.30 P.M.	65.6	30.196	"	"	.05	5.04	123.6	17.12	17.49
									Av'ge.	17.11
" 8	2 P. M.	63.5	30 303	Mannattan	"	.05	5.01	124.2	17.16	17.72
" 9	11.30 A.M.	65	30.074	"	"	.05	5.02	123.0	17.76	18.13
" 10	9.30 A. M.	66	30.192	"	"	.05	4.91	121.2	17.36	17.84
" 11	10 A. M.	67	30.003	"	"	.05	4.97	124.2	17.32	18.03
" 12	11 A.M.	65	29.992	"	"	.05	4.96	123.0	17.08	17.64
" 13	12 M.	65	30.196	"	"	.05	4.96	123.6	16.09	16.65
									Av'ge.	17.67
" 8	2.30 P.M.	64	30.303	N. Y, Mutual	"	.05	4.94	123.0	17.88	18.54
" 9	11 A. M,	64	30.074	"	"	.05	4.31	124.2	17.52	21.02
" 10	10 A. M.	66	30.192	"	"	.05	4,47	123.0	17.20	19.71
" 11	9.30 A.M.	66	30.003	"	"	.05	4.49	120.6	17.92	20.05
" 12	11.30 A.M,	66	29.992	"	"	.05	4.47	124.2	15.68	18.14
" 13	11.30 A.M.	64.5	30.196	"	"	.05	4.44	123.6	16.56	19.29
									Av'ge.	19.46

E. G. LOVE, Gas Examiner

Official Report of Examinations of Illuminating Gas, for the week ending October 20, 1877, made at the Photometrical Room of the Department of Public Works.

Date.	Time.	Temperature of Thermometer.	Barometer.	Gas Company.	Burner.	Pressure at Point of Ignition	Consumption of Gas Rate per hour.	Consumption of Candle, Grs. per hour.	Illuminating Power. Observed.	Illuminating Power. Corrected.
1877.		Deg.				In.				
Oct. 15	11.30 A.M.	66.5	30.291	New York.....	Sugg-Letheby.....	.05	4.92	121.2	17.00	17.44
" 16	10.30 A.M.	68.5	30.058	"	"	.05	5.02	120.0	16.88	16.81
" 17	9.30 A. M.	67	30.248	"	"	.05	4.96	118.2	17.72	17.57
" 18	12 M.	68	30.133	"	"	.05	5.10	124.2	16.48	16.71
" 19	1 P.M.	68	30.007	"	"	.05	4.96	123.0	15.94	16.46
" 20	11 A.M.	69.5	30.000	"	"	.05	5.02	121.8	17.24	17.41
									Av'ge.	17.06
" 15	12 M.	67	30.291	Manhattan.....	"	.05	5.00	124.2	17.04	17.63
" 16	10 A.M.	68	30.058	"	"	.05	5.07	124.2	17.12	17.46
" 17	10 A.M.	67	30.248	"	"	.05	5.02	118.2	18.60	18.24
" 18	11.30 A.M.	68	30.133	"	"	.05	5.02	123.0	16.72	17.07
" 19	1.30 P. M.	69	30.007	"	"	.05	4.98	124.2	16.76	17.41
" 20	10.30 A.M.	69	30.000	"	"	.05	5.00	123.0	17.11	17.53
									Av'ge.	17.55
" 15	12.30 P.M.	67	30.291	N. Y. Mutual.....	"	.05	4.16	124.2	16.20	20.15
" 16	9.30 A.M.	67.5	30.058	"	"	.05	4.24	124.2	16.44	20.09
" 17	10.30 A.M.	68	30.248	"	"	.05	4.21	117.0	16.60	19.22
" 18	10.30 A.M.	67	30.133	"	"	.05	4.20	124.2	15.96	19.66
" 19	2 P.M.	70	29.007	"	"	.05	4.28	122.4	15.87	18.90
" 20	11.30 A.M.	69.5	30.000	"	"	.05	4.48	123.0	16.48	18.85
									Av'ge.	19.47

For the Week ending October 27. 1877.

Oct. 22	10 A.M.	62	29.574	New York.....	Sugg-Letheby.....	.05	4.97	124.2	15.60	16.24
" 23	9.30 A.M.	59	30.074	"	"	.05	4.91	123.0	16.39	17.11
" 24	10.30 A.M.	61.5	30.133	"	"	.05	4.95	124.2	15.80	16.51
" 25	10 A. M.	62	29.960	"	"	.05	5.00	120.0	16.76	16.76
" 26	11.30 A.M.	63.5	30.062	"	"	.05	4.98	123.6	16.08	16.62
" 27	10.30 A.M.	59	30.058	"	"	.05	4.97	123.6	15.52	16.07
									Av'ge.	16.55
" 22	9.30 A.M.	62	29.574	Manhattan.....	"	.05	4.93	124.2	17.12	17.95
" 23	10.30 A.M.	59.5	30.074	"	"	.05	5.10	120.0	18.08	17.72
" 24	10 A. M	61	30.133	"	"	.05	4.95	124.2	17.72	18.51
" 25	10.30 A.M.	62	29.960	"	"	.05	5.00	117.6	18.68	18.30
" 26	11 A.M.	63	30.062	"	"	.05	5.04	117.0	17.92	17.33
" 27	11 A.M.	59.5	30.058	"	"	.05	5.02	123.0	16.38	16.72
									Av'ge.	17.75
" 22	9 A. M.	62	29.574	N. Y. Mutual.....	"	.05	4.60	123.6	17.28	19.33
" 23	11 A.M.	60	30.074	"	"	.05	4.76	123.6	17.93	19.40
" 24	9.30 A.M.	60	30.133	"	"	.05	4.58	123.6	17.70	19.89
" 25	11 A.M.	63	29.960	"	"	.05	4.35	117.6	18.34	20.22
" 26	10.30 A.M.	63	30.062	"	"	.05	4 32	121.8	16.04	18.83
" 27	11.30 A.M.	60	30.058	"	"	.05	4.54	124.2	16.46	18.76
									Av'ge.	19.40

F. G. LOVE, Gas Examiner.

Heat being the important agent in this operation, we can but regret that science has not furnished us with some convenient and ready method of estimating high temperatures; and, until something capable of being reduced to daily practice is discovered, we must be guided by the colors presented by the retorts. This, however, is uncertain, from the fact that when heated retorts are exposed to daylight, the color or temperature appears lower than it really is; or, when they are observed in the dark, the temperature seems higher than it actually is. But constant observation improves the faculty of estimating high temperatures by the shades of color, and continued practice enables the intelligent observer to regulate the working of his furnaces.

The apparatus now used for carbonizing are too familiar to require any description at this time, so we will at once proceed to consider the silent, but powerful, forces that are at work within. There are three of them—heat, cohesion, and chemical affinity, especially the first two, as it is by the mutual action of these two antagonistic forces, which have been appropriately termed the attractive and repulsive forces, that the state or condition of all matter is determined; a preponderance of the cohesive force producing the solid condition of matter; the equilibrium of the two forces the liquid state; and an excess of the repulsive force the gaseous or vaporous condition.

As to the proper degree of heat to be employed in carbonizing coal in gas making we will not consider in this connection, but will first notice its effect upon the charge of coal which, we will assume, is just being introduced into the retort. "The constituents of the coal are, by the first application of the heat, torn asunder, and converted from the solid into the liquid state, by a further increase of the heat these liquids are converted into vapors and gases, which are, in turn, further torn asunder, by the action of higher temperatures, into simpler compounds; at each step carbon, in the solid form, being more or less eliminated, and the liquids and gases becoming simpler in their constitution."

Now, the uniformity of these results depend upon certain unvarying conditions. When coal is distilled at a very low temperature the result is the production of oil vapors; if these vapors be passed through an empty retort or pipe, of considerable diameter, heated to bright redness only, a very small amount of the vapors are decomposed, and the oil vapors pass almost unchanged. If, on the other hand, the retort or pipe be filled with other pipes, each one of less diameter than the last, thereby dividing the large space into a number of narrow annular passages presenting a large surface, and these oil vapors be passed through this when heated to bright redness, the decomposition is very much increased; in fact, only a very small quantity of these vapors escape decomposition, you will perceive it is not owing to any change in the temperature that produces such widely differing results but simply a change of conditions. "This effect of surface in bringing about decomposition, results from the fact that gases and vapors are comparatively feeble conductors of heat, they allow the heat rays, or pulsations sent into them from the heated walls of the retort, to pass freely through them, without being themselves heated to the extent necessary to bring about decomposition, and, therefore, it is only by direct contact that the necessary amount of heat can be communicated rapidly, in order to bring about the decomposition of organic substance when in the vapor state." But, while surface is the medium through which the heat is transmitted to the elements of coal to bring about decomposition, due regard must be had to the temperature of that surface. If the heat be too low, decomposition is not effected; if too high, decomposition is carried too far, and the gases become too simple in their constitution. Seeing, then, that the heat limits are comparatively narrow, within which carbonization must be conducted, it is evident we must have some certain means or arrangement connected with our retorts by which we may secure both the requisite amount of

properly heated surface, and insure the contact of the vapors with that surface, if we ever attain the results that experiment seems to indicate as being within easy accomplishment.

This brings us to the consideration of the apparatus now employed, and more especially the mode of using them in this most important department of gas making. The clay retorts now so universally adopted, I think are altogether satisfactory so far as durability is concerned, any lack of efficiency is, in my opinion, almost, if not entirely, owing to a want of subdivision of the processes which actually take place in every coal retort. As we have already seen coal is always distilled at a low heat, and the condensable vapors are subsequently converted into permanent gases.

We may now consider whether it is possible with the means employed to maintain nearly a uniform temperature, which is indispensable to uniform results. In practice we take a retort, say 12"x20"x8'6" long, heated to bright redness, into which we shovel, as rapidly as possible, from 150 to 200 pounds of coal; the consequence is that, within from twenty to thirty minutes, the temperature of the inner surface of the retort is so much reduced that it is incapable of doing little more than change the solid elements of the coal into the liquid or vaporous state; this process, however, goes on very rapidly, even at the reduced temperature, which fact may be easily determined. With a suitable apparatus I found, by a number of experiments recently made on good Pittsburgh coal, that with the retort heated to the lowest visible red heat, that the volatile matter of the coal was expelled at the following average rate. At the end of the first hour after introducing the charge that fifty-eight per cent. of the whole gas-producing matter had passed over; at the end of the second hour eighty-five per cent.; and at the end of the third hour ninety-six per cent., or nearly the whole of the volatile matters, and with the cannel coals the rate was even more rapid. This, then, being the fact, we can very readily foresee what must of necessity be the result. While the heat is so low the great bulk of the volatile matter is driven over, and, failing to meet with heated surface to decompose them, these vapors are carried forward and gradually condensed into liquids, whose composition is the very elements that constitute good illuminating gas; with different treatment, at the proper time, these same vapors could be measurably converted into permanent gases. This low temperature continues till the charge of coal, by contact with the bottom and sides, and by radiation from the top of the retort, has become heated to redness, which usually requires from one to one and a-half hours, at which time things are in good condition for efficient work. With a large per cent. of the gas-producing material wasted, so far as lighting is concerned, we have now attained the most favorable conditions possible with the retorts as now used for carbonizing coal; and could we maintain just this condition of things, to the end of the charge, it would be well. But, no; as the outer surface of the coal in the retort has become sufficiently heated to melt and form an envelope, or crust, of coke, and this crust having parted with its vapors, is soon raised to a high temperature; through this countless jets of vapor are constantly forced from the enclosed portion of the charge. The temperature of this crust is not only increasing, but the heat is penetrating to a greater depth; the vapors from the interior having to pass through this excess of heat, decomposition is carried too far, carbon is deposited in the form of fine particles, exceedingly imparting to the coke that peculiar white, metallic lustre always observed in coke from hot retorts; and the gas produced from this time to the drawing of the charge becomes gradually poorer in quality. This, however, I think should not be so. As the coal put into the retort was all alike it seems to me that, with proper manipulation, the product should be the same in quality through the entire process.

For the purpose of testing the correctness of this theory I recently made fifteen experiments, with suitable apparatus, where the coal was distilled at a low heat, and the resulting vapors passed immediately through a well-heated retort, or chamber, for decomposition, and with observations every fifteen minutes during the whole four hours the charge remained in, there was a remarkable uniformity in the quality of the gas while the temperature was uniform, but with any considerable increase of the heat of the decomposing retort, there was a larger proportionate yield, but the gas was of a poorer quality; on the other hand, after the heat had been increased, if it was reduced again to the original temperature, there was a corresponding decrease in the yield, but with a perceptible improvement in quality.

Referring again to the defects in our system of working retorts, I would remark that the process of gas making, being fractional in its character, cannot certainly be commenced and completed at the same time; the circumstances and conditions under which the first part of the process is most satisfactorily accomplished, are not at all adequate to its completion. As has already been said, when we crowd two hundred pounds or more of cold coal into an ordinary sized retort, be it ever so hot, the temperature is soon reduced below that at which decomposition is effected. Urge the furnace as you may yet the heat remains down. Now, by experiment, it appears that this low heat is just about as efficient in liberating and driving over the volatile elements of the coal as a higher heat, while it falls short of decomposition, hence, there must be more or less loss, just in proportion to the deficiency in heat. This difficulty may be overcome, to a very great extent, by dividing the processes, and adopting a different plan of working, so that we may avoid the necessity of trying to perform three delicate operations, in the same vessel, and at the same time.

In regard to the proper heat to be employed in carbonizing coal I think there can be no fixed rule that will apply to the ever-changing circumstances. It has been assumed, from the beginning of this paper, that the coal should only be subjected to a comparatively low heat in gas-making; this I am aware is contrary to the opinion now entertained, and directly the opposite of universal practice; nevertheless, after many experiments and careful observation, I feel warranted in the conclusion that several most serious difficulties may be greatly alleviated, if not entirely overcome, by a change of practice in this respect, some of which I will enumerate—the excessive deposit of carbon in the retorts; reducing the quantity of tar; removing the naphthaline nuisance; and by making it possible to control the heat within certain limits. And while I would discourage the application of high heats directly to the coal, I would, on the other hand, say that a reasonably high heat is absolutely necessary to the treatment of the vapors arising from distillation at a low heat. In order to ascertain, if possible, the best heat, I have made a number of experiments with the following results, taking the average of several successive trials (with good Pittsburgh coal) the heat being what we will call bright redness, or, as I suppose, from 1800° to 2000°F. The yield was at the rate of 10,600 cubic feet, of 20·5 candle gas to the ton (2240 lbs.) of coal. Then with the same coal and same apparatus, but with the heat of the retort raised to a vivid red, bordering on whiteness, supposed to be about 2600°, yield was at the rate of 13,600 cubic feet, of 10·27 candle gas, to the ton of coal, which would seem to indicate that too high heats are not desirable; it is a fact worthy of notice that, while the yield in the above experiment was increased twenty-eight per cent., the quality of the gas was brought down just one-half.

This effect of high heats on the rich gases from coal is owing to the fact that, when two gases or vapors unite chemically, a contraction of volume ensues; take, for example, olefiant gas (C_2H_4) composed, as it is, of two volumes of carbon and four volumes of hydrogen—these six volumes, when chemical union takes place, contract into two volumes, or just one-third; or take naphthaline, ($C_{10}H_8$) whose proportions are ten volumes of carbon and eight of hydrogen, these eighteen volumes, when chemically united, occupy the space of only two volumes, or one-ninth its uncombined volume.

Now, if these gases rich in carbon are subjected to excessive heats, dis-association takes place, the carbon is deposited in the solid form, and, the hydrogen being set free, expands into its original volume.

In conclusion, I will take occasion to say that I am not disposed to undervalue the results that are being had every day in all well-managed gas works, for I honestly believe that the results now obtained are about as good as can be had with the apparatus now used, and the mode of carbonizing; and, at the same time, I am just as honest in the belief that, with modified apparatus, and with better treatment of the coal, that the average yield may be very considerably increased in volume, and yet quite as good in quality.

I thank you, gentlemen, for your courtesy and attention while I have presented these commonplace and well-known facts; and as I accepted the invitation to read a paper, I have felt bound to fulfil my promise, although I could not hope to say much to you that was new on this subject.

At the close of the reading of Mr. Rankin's paper the President said:

Gentlemen; the paper and the writer are before you, and you can ask him any questions that you like.

Mr. Coggshall—In ascertaining the candle-power of the 20½ candle gas from 4·73 to the pound, I would like to ask whether it was by bar photometer or jet?

Mr. Rankin—Bunsen's photometer, with the London burner, a part of the time; and a part of the time I used Sugg's standard burner. I used Sugg's bat's-wing and fishtail, and found the ordinary fishtail burner gave a better result than the London burner did.

Mr. Littlehales—I would like to ask if the results were obtained by any method of superheating the vapors from the ordinary retort?

Mr. Rankin—The coal in these experiments was subjected to a low temperature, not with a view of converting the solid elements into gas at once; but while the elements were still heated, they were passed into a second retort or vessel, when they were decomposed.

Mr. Littlehales—That is what I supposed I have been experimenting a little myself in this same direction. I set an iron retort on the top of the retort arches, and heated it with the waste heat from two benches of fives, and passed the gas from the hydraulic main through this retort. During these experiments I scarcely found a trace of tar, nearly the whole of the hydrocarbons having been decomposed, and converted into dry carbon in the form of soot or lamp black, and probably hydrogen gas. I had to abandon it temporarily, because the stoppages were very frequent; but I think that was mainly because the supplementary retort was too hot. I have often noticed, and doubtless you all have observed the same thing, that, when a retort is just charged, a great many heavy vapors come off, which are so dense that they will scarcely burn at the mouth of the retort; and there can be no doubt at all but a large proportion of the vapors distilled from the coal, near the mouthpiece end of the retort, are pushed forward to the ascension pipe by the gas which is being more rapidly generated at the back end and middle of the retort, and are never raised to a sufficiently high temperature to be made into gas at all. There is some difficulty in working the system I have mentioned. In the first place, the heat of the supplementary retort is a very important matter, just a dull red would, I think, be best; and then, again, the size of the supplementary retort, or the area of the superheating surface, in proportion to quantity of gas made, and it is also absolutely necessary to have the time of charging the retorts so arranged that they shall follow each other in regular succession, and so as not to have too many charged at once, or else there is a tendency to overcarbonize the coal at one time, and the gas itself is decomposed, and at another time it is not sufficiently carbonized. I feel sure, from experiments which have been, and are being made in vari-

ous places in this direction, that the time is not far distant when some such method as this will enable us to obtain such a yield of gas from the coal that very few of us at present anticipate.

Mr. Battin—I would like to ask Mr. Rankin if there was an increase in the quantity of coke left after this method?

Mr. Rankin—I would say, in regard to the coke, that it presents a very different appearance from the ordinary coke. Its heating qualities are about the same, but it presents a very different appearance. It is of a very dark color compared with the other coke. It has none of the bright metallic appearance of that made in hot retorts, nor do I think it possible to make that kind of coke without the deposition of carbon.

Mr. Coggeshall—You say that when you were producing six feet to the pound you distilled the coal at a low temperature, then ran it into a retort of a higher temperature. I can't account for the decreased illuminating power. I tried an experiment with four retorts, running the gas into the top retort, and produced 19½ candle gas. I did't get the quantity of gas you did, but that was because my heat was too great.

Mr. Rankin—A series of experiments was made under a back pressure of one half an inch as near as I could work it, and I may say as a result, I am in favor of back pressure.

Mr. Gray—What was the length of each of the charges?

Mr. Rankin—Four hours. I may say here that I don't apprehend any trouble from hard tar or pitch, for my impression to-day is, that with the low temperature you will not produce real tar—it will not be tar. Therefore it is not worth our while to anticipate trouble where it don't exist. You can't produce tar under a certain temperature.

Mr. Wood, of Syracuse—Is not your process the same that was in operation some time ago at Yonkers, New York.

Mr. Rankin—I do not know, Sir.

Mr. Wood—If so, I will say that it has been abandoned there.

Mr. Rankin—The experiments I allude to were in view of utilizing the waste heat. From experiments made during the past few months, I am led to conclude that there is enough waste heat in every retort house to do all the distilling, and with a slight modification of the apparatus now in use, I think there can be a considerable yield of equally good gas. But I have not satisfied myself on all points. I expected to try experiments on a very extended scale, but in a recent enlargement of our works it became necessary to throw a portion of our apparatus out of use, so that I failed to carry the matter as far as I otherwise should have done. I simply give my results as suggestions, and hope that some one else will be interested in the matter, now that there is the ability here to carry the investigation so much further.

Mr. Helme—It seems to me that with the low temperature for four hours, you wouldn't get all the valuable vapors out of the coal.

Mr. Rankin—But I am satisfied I did Sir, and I think you would find it well worth your while to try a few experiments in regard to low heats, upon the gas producing elements of coal. A gentleman remarked to me once that he was satisfied that with low temperatures he could take all the gas out of the coal, and if he had red heat for four hours I will admit that he could. You will be surprised at the rapidity with which these volatile materials pass out of the retorts. But what I gave you in regard to that in the paper is absolutely true—not the result of one observation nor ten, but a series of experiments extending through three or four months.

Mr. Helme—What was the size of your retorts?

Mr. Rankin—I use a five inch wrought iron retort, so that I can raise or lower the temperature with impunity.

Mr. McMillan—Which was the best coke—that made from low heats or from high heats?

Mr. Rankin—I havn't compared them very carefully. All the coke made from high heats is quenched with water, and is, of course, very much heavier. In my experiments I used no water at all, so that I had every particle of coke. I weighed it both before and after, and I have all the data in my memorandum book at home.

The President—You weighed it in an incandescent state?

Mr. Rankin—Yes, sir.

Mr. Helme—Would you use two or three retorts at low temperature, and two or three at high, or how?

Mr. Rankin—My opinion in regard to the matter would be to use the ordinary retorts in the benches we now have, and in the place of D retorts would have partition walls, say two inches thick, and would work through retorts—take the gas in at one end and out at the other—would charge at both ends with the coal. I am satisfied from experiments that I made, that it is impossible to decompose vapor given off from many retorts, until the mass of coal has attained that degree of heat necessary for decomposition.

Mr. Helme—It does seem to me that with retorts of the ordinary size, and running a full charge, at a low temperature, it would be impossible to get the gas out of the coal in four hours.

Mr. Rankin—Notwithstanding that it is true that it can be done.

The President—If there are no more remarks to be made on the subject of this paper we will pass on, and listen to a paper prepared by Mr. Thomas Curley, which at his request will be read by the Secretary.

On Naphthaline.

By Thomas Curley, of Wilmington, Del.

Mr. President and Gentlemen: At the request of our worthy Secretary, I have prepared a few remarks on the system which I employ for the purpose of preventing, or at least, lessening the deposition of naphthaline crystals about our works, particularly in the inlet pipes to the holders, which had formerly given us so much trouble.

In doing so I considered it necessary to go into the history of this substance and its characteristic properties, except to revert slightly to its formation or production. In the destructive distillation of coal in gas works, naphthaline vapors are always given off, until the greater portion of the bituminous matter of the coal is expelled; whether the heats be what we term high heats or low heats, those vapors are always given off, and will crystallize or not, just in proportion to the temperature of the retorts, and the quantity of light vapors in the gas. If the temperature be moderate or low, a considerable quantity of light hydrocarbon vapors will escape decomposition, and their presence in the gas mixture will prevent the naphthaline vapors from crystallizing; if on the contrary, our heats are extremely high, the greater portion of those light vapors is decomposed, forming new and stable compounds, which have no neutralizing effect on the naphthaline vapors; another portion is absorbed by the tar, the remaining portion, which is held in suspense by the gas, is too small in volume to counteract the tendency of the naphthaline vapors to crystallize, hence the trouble to which we are so often subject. Under these circumstances what is best to be done? We cannot return to low heats, it is unprofitable; we cannot afford to rely on the tar for the extraction of those vapors out of the gas, we risk too much in doing so, because the tar will make very little distinction between the naphthaline and light vapors; it will absorb both. The free use of coal oil is undoubtedly an effective remedy, and will prevent the formation of crystals, while adding to the illuminating power of the gas; and the only question to be considered, is its safety and cost. The process used in our works is in one respect the result of necessity. I found the inlet pipes to the holders stopped so frequently that I was compelled to use steam every few days, that by dissolving it we might pump it out. The trouble and inconvenience became so great that I resolved to expel from the tar the benzol and naphtha series of hydrocarbons, and mix them with the gas, knowing them to be great solvents of naphthaline. With this view I constructed a small still and separator; in the still I placed a small coil of steam pipe, and caused the tar I made, to pass through this still. I examined the inlet pipe immediately before heating the tar, and I found three-quarter inch of crystal on the inside. I then heated the tar to about 170° and allowed its vapor to pass on and mix with the gas. After the expiration of twenty-four hours I again examined the inlet pipe and found it as clean as when new, showing that the expelled vapors had dissolved the crystals which had been formed. This was in January, 1876. Up to this time we used from 5 to 8 per cent. of Cannelton Cannel, but since then we discontinued its use altogether.

In the process the tar (after the expulsion of the light vapors) and liquid flow on to their respective receptacles. It is automatically arranged and requires no attention, comparatively speaking. We get the light vapors out of the tar without any expense, and utilize them first as solvents, and secondly as illuminants. It is well known that even small quantities of light hydrocarbon vapors add materially to the illuminating power of the gas, and hence, the greater the quantity the tar can yield, the less the danger of naphthaline crystals, and more illuminants will be added to the gas. But, as I have already intimated, I may carry the process of carbonization to such an extent, as to leave very little of those light vapors in the tar. In such a case there will, of course, be no substance left in the gas, of a solvent natnre, to counteract the crystallization of the naphthaline vapors. Under such circumstances they will, undoubtedly, crystallize.

But such policy I consider as being very doubtful indeed. I would not, on any account, decompose all of the light hydrocarbon vapors in the process of distillation; for by doing so, I would first rob the gas of one of its principal elements of light, and leave the naphthaline vapors free to crystallize according to their nature; second, by the breaking up of these vapors they become resolved into numerous light carbides, the principal portion of which being marsh gas and hydrogen, and destitute of luminosity, only necessitates the subsequent enriching of the gas by cannel or oil.

Gentlemen, I hold that by proper manipulation and consideration, our principal coals (*e. g.* coal from Westmoreland region) need no enriching material to maintain a yearly average of 10,800 feet to the gross ton, and a power of 17 candles. This has been my experience, and for aught I know, others may have far exceeded it. What I state with regard to our results, be they favorable or not, can be substantiated by the President and Directors of our company. The comparative result, thus far obtained in our works (taking the years 1875 and 1876), shows a gain of 2 per cent. in yield, and of candle power of 48–100; the average candle power for 1875 was 16.60, and for 1877 was 17.08. Our freedom from naphthaline crystals is one of the most gratifying results obtained, and was unknown to us before.

In 1875 we used cannel; in 1876 we used none, showing conclusively that it is our advantage to continue the process. I don't profess to have discovered the fact that benzol and naphtha vapors will dissolve naphthaline, or add to the illuminating power of the gas; but only the simple and inexpensive method of getting and utilizing those vapors.

Whatever important and startling discovery there may be in the future, in the interest of gas manufacturing, one thing is imperative at the present time, and that is to look after, collect and utilize everything committed to our charge, whether it be the leakage of mains, services, meters, etc.; or the constructing and setting of retorts, their temperature, and that of our condensers or purifiers.

As the mass of matter is made up of atoms, so we will find that our success depends very much on the aggregation of small things.

At the conclusion of Mr. Curley's paper the President said? Gentlemen—I have another paper on the same subject, which has been prepared by Captain

Dresser, and as the Captain is a very modest man, if he desires it, I will read the paper for him.

Capt. Dresser said: Gentlemen—I am afraid you will all get tired of this subject before you get through with it. However, I haven't prepared much, and I want to state, to begin with, that it isn't any of it mine. [Laughter.]

On Naphthaline.

C_{10} H_8.

By Capt. G. Warren Dresser.

Mr. President and Gentlemen of the American Gas-Light Association: In presenting for your consideration, the results of some study of this subject, it is my aim to collect and lay before you known facts rather than to advance any theories new in themselves, or novel in their application to solving the many difficulties which this same $C_{10}H_8$ entails upon the gas manager.

There may be some of those present who are entirely unfamiliar with the appearance of this beautiful yet troublesome article. Happy is the gas engineer who is equally unfamiliar with the results following the deposit in his pipes and services.

It is composed, as the chemists tell us, of carbon and hydrogen, and its chemical formula is $C_{10}H_8$. In studying the many compounds of C and H, which result from the various processes of gas manufacture and its residuals, the part that *temperature* plays in determining the composition of the hydro carbons resulting, is a most important one. This is generally understood, and it is almost an axiom in gas making, that "high heats produce naphthaline." At any rate whether this be true or not, it is generally accepted as one explanation of its cause. Now, if this was the only cause for its formation, it would be easy to prevent it by reducing the temperature of distillation, but here again the manager would be confronted with the question from his directors, "Why do you not get more gas from a ton of coal," and he often would have to determine which was the greater nuisance, the occasional appearance of the $C_{10}H_8$ or the continued inquisitiveness of the "economical director." ever anxious to assist the engineer in bringing up his results. We know, however, that in many a well ordered works the annoyance is not confined to either one of these cases. Let us now take up the subect:

First—By trying to understand the true chemical character of the body.

Secondly—By studying what has been done by others in determining the cause and manner of formation of the substance.

Thirdly—By stating the results of various methods of preventing and removing the many annoyances resulting from its formation or deposition, both about the works and in the pipes and services in the street.

First then as to the chemical character of naphthaline.

In the edition of "Towne's Chemistry," issued in 1347, we find the following under the head of "Naphthaline." When in the distillation of *coal tar* the *last* portion of the *volatile oily product* is collected apart and left to stand, a quantity of solid crystalline matter separates, which is principally composed of the substance in question. An additional quantity may be obtained by pushing the distillation until the contents of the vessel begin to char; the naphthalene then condenses, but is dark colored and impure. By simple sublimation once, or twice repeated, it is obtained perfectly white. In this state naphthalene forms large, colorless, transparent, brilliant, crystalline plates, which exhale a faint and peculiar odor, which has been compared to that of the narcissus. Naphthaline melts at 176° to a clear colorless liquid which crystalizes on cooling; it boils at 413° and evolves a vapor whose density is 4.528. When strongly heated in the air it inflames, and burns with a red and very smoky light. It is *insoluble* in cold water but soluble to a slight degree at a boiling temperature. Alcohol and other easily dissolve it. A hot saturated alcoholic solution deposits fine irredescent crystals on cooling.

"The history of the formation of naphthaline is rather interesting. It is perhaps the most stable of all the complex compounds of carbon and hydrogen. In a vessel void of free oxygen it may be heated to any extent without decomposition, and indeed where other carburets of hydrogen are exposed to a very high heat, as by passing in vapor through a red hot porcelain tube, *a certain quantity of naphthaline is almost invariably produced.* Hence its presence in coal and other tar is *mainly dependant* upon the *temperature* at which the *destructive distillation of the organic substance has been conducted.* Lamp black very frequently contains naphthaline thus accidentally produced." (Townes Chemistry, 1847 edition, page 436.)

Thus we see that thirty years ago points were hinted at, which to-day are taken up again, or are rediscovered and presented for the consideration of the gas manager. It is true that but comparatively little was known of many of the coal tar products at that day, as the same writer says in another paragraph, speaking of the compounds of the same: "The composition of these substances is as yet very uncertain."

But in the thirty years that have elapsed since the issue quoted from above, the chemist has not been idle. The marvellous strides made in the study of hydro carbons is well known to most of you. In fact so great has been the mass of information acquired in this particular branch of the subject, that to the ordinary gas manager the array of facts is almost bewildering. One of the latest and best works on this subject is "Roscoe's Lessons in Elementary Chemistry," printed in London, 1877. It has the advantage of being a small book but it is very full. In speaking of the *carbon compounds* it says, "The first striking peculiarity which the carbon compounds exhibit is their extraordinary number, those already known far exceeding all the compounds of the other elements taken together, and new ones being daily brought to light." The second peculiarity is that they are almost all of them formed by the union of carbon in different proportions with one or more of three other elements, viz., H., O., N. The cause of the multiplicity is to be sought in the fundamental and distinctive property of carbon itself, viz., the power of *uniting with itself* to form complicated compounds." To those desirous of familiarizing themselves with what has been found out up to date, regarding the hydro carbons, we would suggest a careful study of this work.

In the tenth edition of "Towne's Chemistry, published in 1875, we find the following, in addition to what was quoted above from the edition of 1847:

"Naphthaline results also from the decomposition of toluene (C_7H_7), qylene ($C_8H_4(CH_3)_2$, cumene $C_9H_3(CH_3)_3$ by a red heat (each of these are benzene compounds containing respectively 7, 8, and 9 atoms of carbon). It is also obtained by passing the vapor of benzene (C_6Hc) or anthracene ($C_{14}H_{10}$) through a red hot tube. In gas making it is obtained as a by-product *from reactions similar to the above.*"

Further on we shall refer to examinations showing this to be the case.

Roscoe, page 425, edition 1877, thus gives the chemical character of naphthalene. "This hydrocarbon occurs in large quantity in the heavy oils, and is formed when the vapors of benzene and many other substances, even alcohol and acetic acid, are led through a red hot tube. Naphthalene crystalizes in large white pearly plates; it melts at 80° C. (176 F.) and boils at 217° C. (423 F.) but *sublimes* at a *lower* temperature. The carbon atoms in naphthalene are connected together in a similar way to those in benzene. The eight atoms of hydrogen in $C_{10}H_8$ can be successively replaced by chlorine, but naphthaline can combine directly with chlorine, and a series of further substitution products can be obtained both from the dichloride ($C_{10}H_8Cl_2$) and the tetrachloride $C_{10}H_8Cl_4$, so that these chlorinated derivatives of naphthaline are very numerous. By the action of nitric acid upon naphthaline four nitro substitution products are formed."

From this may be gathered an idea of the chemical character of our troublesome guest. While this body is insoluble in cold water and but slightly so in boiling water, it is very soluble in steam. In fact is one of the most powerful solvents of it, but the naphthaline will recrystalise as soon as the temperature is reduced to the proper point. As an *example* of this the deodorizing apparatus of the Manhattan Gas-Light Company of New York for deodorizing the foul lime, has connected with it a sewer of about 1000 feet in length, into which the air is drawn through the foul lime in the boxes, after the covers are raised is forced by a fan wheel, and from which this air is passed through other boxes containing ventilated foul lime.

In a communicated article to the American Gas-Light Journal, vol. 24, page 113, is given an account of this, in which the following occurs that is germain to our present subject. "The sewer pipe leading to the deodorizing house, is coated on the inside to the extent of five or six inches in depth, after a years use, with crystalized naphthaline, which is *removed* by introducing a jet of steam, and blowing it through by the exhauster. Upon one occasion we filled the *washer* above referred to (this washer was for washing the air after it was drawn through the boxes) with slats, for a better distribution of the water, but after deodorizing four or five boxes we were obliged to remove them, because we found that the washer was *completely choked* with crystalized naphthaline. This would seem to indicate that a large quantity of naphthaline is arrested by the lime in the purifying boxes, for there is no other source whence it could be derived during this operation.

I have seen the application of steam referred to, and the result was a most beautiful "snow storm" of pure finely divided crystals of naphthaline in the deodorizing house. It was deposited to a depth of several inches upon everything in the room. The flakes were not seen until the solution of $C_{10}H_8$ in the aqueous vapor had reached about five or six feet above the floor, when the temperature was sufficiently reduced for recrystalization to take place, and the "snow" resulted.

Here it would appear that there was no absence of naphthaline, and yet the company in question do not run very high heats, and have almost no trouble in the street lamps, and none in their services.

A single instance occurred within a year where a large establishment, a brewery, complained of "no gas." There being no complaints of naphthaline any where at the time, in all their district, this never occurred to them as a possible cause of the obstruction. The meter was examined and the service pipe blown out, but no benefit followed. The service was uncovered and found all right. Following the main house pipe into the building it was found that it passed for a distance of some feet under a large room used for storage of ice. Upon locating the stoppage it was found to coincide with the portion of the pipe beneath the ice. Upon the pipe being disconnected it was found *filled* with naphthaline, completely preventing the passage of the gas into the building

Here, certainly, condensation must have been the cause. Why high heats produce $C_{10}H_8$ in gas making is evident from the nature of it; it is obtained by the highest heat in distilling coal tar. It is the last portion of the oily volatile product that left apart to stand (and cool) gives a quantity of solid crystalized C,O, H_8 and an *additional* amount may be obtained by pushing the distillation still more. It is formed directly in the laboratory by passing various of the hydrocarbons through red hot tubes. I will now give you a translation of a paper, to which was recently awarded the first prize by the "Societè Technique du Gaz d'eclairage en France." Its author, Mr. Lucien Bremond, is the manager of the Versailles Gas Works, and has certainly brought to this subject much careful thought and practical investigation.

Paper of Mr. Lucien Bremond to the "Societe Technique au Gaz d'Eclairage,

Naphthaline $C_{10}H_8$ is a body whose study has caused very numerous and varied researches, both as to the cause of its production and the means to avoid it.

It is, in fact, the greatest nuisance about a gas works, since it produces an obstruction of the pipes

about the works, or in the street mains and services, meters, etc.

Without entering into the details of the investigations upon this subject in France, in England, and in Germany, I may say that the conclusion arrived at in them all is this: "The appearance of naphthaline, and especially its disastrous effects, date from the time when the distillation of pit coal at high temperatures was commenced. The only remedy found for this evil is to secure a perfect condensation, and to leave the gas as long as possible in contact with the tar in the pipes of fabrication.

This double conclusion outside of the sanction in practice is verified theoretically. In the distillation of coal at high temperatures, on the one hand we effect the more or less partial distillation of the tar (for every one knows that in the manufacture of naphthaline in the arts it is obtained by distillation of tar). On the other hand, the temperature of the retort permits the development of igneous reactions, so well and deeply studied by Mr. Berthelot, and which I propose to examine in passing, so far as they relate to the production of naphthaline. First of all it is necessary to enumerate the gases of the hydrocarbon series, which analyses shows to be contained in illuminating gas. They are

Marsh gas $C\,H_4$
Acetylene C_2H_2.
Ethylene C_2H_4,
Propylene C_3H_6.
Butylene C_4H_8.

Those igneous reactions, of which one of the most interesting (due to Mr. Sannsure) is the production of $C_{10}H_8$, in the decomposition of the vapor of ether or of alcohol by a red heat, reduce themselves to four principal combinations.

1st. Condensation polymerique.

2nd. Combination of carburets between themselves and with hydrogen.

3rd. Inverse decomposition.

4th. Reciprocal decomposition.

By polymerique condensation we find that acetylene can produce benzine or styrolene.

$3C_2H_2 = C_6H_6$
Acetylene Benzine
$4C_2H_2 = C_8H_8$
Acetylene. Styrolene.

The benzine thus produced heated with the acetylene will give by direct combination styrolene

$C_6H_6 + C_2H_2 = C_8H_8$
Benzine. Acetylene. Styrolene.

By decomposition of the hydrocarbons into hydrocarbons with small proportions of hydrogen and into hydrogen, or by inverse combination

Ethylene gives hydrogen and acetylene

$C_2H_4 = H_2 + C_2H_2$

Lastly, by reciprocal displacement of the hydrocarbon groupes we reproduce benzine and styrolene.

$C_{12}H_{10} + C_2H_4 = C_6H_6 + C_8H_8$
Diphenyle Ethylene Styrolene Benzine

Thus with ethylene, olefiant gas we produce acetylene, with acetylene we produce benzine and styrolene, which gives us all the elements for producing naphthalene. In fact,

1st. Styrolene and Acetylene give Naphthaline.

$C_8H_8 + C_2H_2 = C_{10}H_8 + H_2$

2nd. Styrolene and Ethylene give Naphthaline.

$C_8H_8 + C_2H_4 = C_{10}H_8 + 2H_2$

The two reactions serve to explain the formation of naphthaline during distillation at high temperatures.

More of them may be found in the memoir of Mr. Berthelot, published in the Bulletin of the Chemical Society, but they are more complex, and their study would exceed the limits of our paper. Here then you have the two causes of the production of $C_{10}H_8$ by the distillation of coal at high temperatures. First more or less, perfect distillation of the tars, and

2nd, Igneous reactions. We shall see further on that a third cause for it exists.

As to the remedies proposed against the nuisance of $C_{10}H_8$ they are all based upon its solubility in tar, such as the prolongation of the contact of the gas with the tar (worm?), or upon its solubility in the oils of naphtha, petroleum, etc., distilled with the coal and easily condensible, or spread out over the purifying material. This was the state of the question when I undertook these researches, under the direction of the learned engineer of the Parisian Company, Mr. Arson, and there it is to-day.

Those examinations which I review today, bear upon two points, viz.:

1st. What is the influence of aqueous vapor injected into the gas by the exhauster under a pressure of five atmospheres?

2nd. Is a change in the electric state of the gas one of the causes of the production of naphthalene?

The series of experiments to which I invite your attention, allow me to reply that the injection of aqueous vapor into the gas *would* give birth to some naphthaline.

As to the influence of a change in the electric state of the gas, I have been able to find nothing which permits me to suppose that this is a cause of the production of naphthaline. Neither by passing electricity into the gas by a very ingenious machine constructed under the direction of Mr. Arson, nor by causing an electric spark to be disengaged by means of a very strong Rumkorff coil, in a tube filled with gas during an entire day. I was unable to produce by either of these two means a single atom of naphthalene. The question remained just as it was, viz., Does naphthaline pre-exist or not with gas? From new observations and new experiments necessitated by a considerable production of $C_{10}H_8$ in the works which I manage, I have been led I think, if not to a categorical answer to this question, at least to the discovery of another cause of the production of naphthaline, and consequently to a means of preventing this production.

But what these observations and experiments do enable me to say is this, that if $C_{10}H_8$ pre-exists in the gas *after the apparatus for condensation and purification* (which I do not believe), its deposit is due to a physical phenomenon. While if it does not pre-exist after the purification of the gas, its deposit is due to a chemical phenomenon. Now, these observations and experiments bear upon a single point, that is, that naphthaline *is produced always* when there is a condensation of the aqueous vapor contained in the gas—that this deposit is preceded by the phenomenon of the condensation of the water, and that gas, absolutely dry, deprived of its aqueous vapor as much as is possible, for the absolute dessication of the gas is very difficult to accomplish; that gas, I say, absolutely deprived of the vapor of water, does not deposit naphthaline in the ordinary conditions of temperature and pressure.

If then naphthaline does pre-exist in the gas after its purification; if its crystaline deposit is due to a physical phenomenon, if the crystalization is preceded by the condensation of the vapor of water contained in the gas, it obeys a law which I might formulate thus:

"In a mixture of several vapors, when one of the vapors attains its point of condensation, it displaces the point of condensation of the vapors with which it is mingled."

This law has as yet been neither established nor verified. I have submitted it to the consideration of experienced physicists and I may say that it is now the subject of study by a learned professor of Physics, a friend of mine, whom I cannot otherwise designate than to say he is the director of the laboratory of Physics of the High Studies of the College of France.

I abandon this hypothesis and return to that of the *non* pre-existence of naphthaline in the gas, which is the one that I maintain.

Naphthaline does not pre-exist in the gas, for in all the apparatus which the gas traverses, it is submitted to a temperature much lower than the condensing point of $C_{10}H_8$, which falls to 79° Cent. (176 Fahr.) and boils about 216°—220° Cent. (419—428 Fahr.) under a pressure of naphthaline (especially that which the gas could hold in solution up to its point of saturation) would then be deposited at a definite point, and the gas after having abandoned the greater part of what it contained, in consequence of the realization in all gas works of the remedy previously cited, which consists in leaving the gas as long as possible in contact with the tar; this gas, I say, were it still saturated with the vapor of naphthaline at the entrance of the purifiers, in spite of an energetic refrigeration in winter for example, would no longer deposit considerable masses at determinate points of its course, as I will show. Crystalized naphthaline makes its first appearance in gas works that distill at high temperatures, under the covers of the first purifying boxes, that is to say, before its second passage into the purifying boxes, if it enters into the last from above, or in the pipes which precede them if it enters from below. Up to that point little or no naphthaline is sublimed because in each works, even those where the condensation is most perfect the gas always arrives at the first purifiers holding in suspension some tar, which is totally arrested by the first filter. At the outlet of the purifiers naphthaline is deposited, when the pipes are at a temperature sufficient to allow the vapor of water contained in the gas (either that coming from washing the gas which the temperature of the reaction in the purifiers has not allowed to condense, or that due as we know to this same reaction) to condense either in whole or in part. Then the pipe to the station meter is less and less obstructed, the gas abandoning also less and less of its aqueous vapor. At the station meter the gas is saturated anew with aqueous vapor, and while the inlet has its section almost free, its outlet is closed or comes to have a section much more restricted by the naphthaline that collects there, the necessary conditions being fulfilled for the condensation of aqueous vapor. The same thing happens at the inlet and outlet of the holders and the outlet governors, but to an extent less perceptible, inasmuch as the entire mass of the gas does not come in immediate contact with the water. These observations are the result of notes made in the works which I direct when, as I have said, the production of naphthaline is considerable; this is due perhaps to a cause which I have not made known here, although it may be a strong argument in favor of the theory which I sustain.

I will cite one case. On the 1st of December a new station meter with new outlet and inlet pipes was placed in service. Although this meter measured each day a volume of gas considerably above that for which it was built, still it absorbed only fifteen millimeters of pressure. On the 1st of January following, one month after, it absorbed 170 millimeters of pressure. Its outlet pipe was lined all around (bushed literally) with $C_{10}H_8$, its inlet though spangled with naphthaline was relatively free from it. It is useless to say that the meter was filled with pure water. Why are the covers of the two purifiers spangled with sublimed naphthaline on the inside? Why is the surface of the material in the boxes covered with naphthaline? It is because the gas on leaving the boxes, saturated with aqueous vapor change of velocity finds a condenser with a large surface abandons a part of its aqueous vapor, and the proof that it abandons a part of this vapor is, that if the gas enters from above the surface of the material is damp, wet, and that in descending through the interstices, the dampness diminishes, disappears towards the middle and reappears again below near the bottom, which is a new condenser cooled by the soil, or the current of air in the cellar under the purifiers. Leaving these naphthaline reappears, obstructs the outlets of the boxes, and its deposit, as I have said, goes on diminishing to the apparatus, which follows where the gas reabsorbs all the quantity of water which it has given up *en route*, gives it up again by condensation, and will deposit anew more naphthaline in some conditions, such that the first deposits of it which have taken place would not permit us to suppose that it existed still. The analysis of illuminating gas does not show the presence of naphthaline. I speak only of such analyses as have been made up to the present time, and do not refer to the new communications made by Mr. Berthelot to the Academy of Sciences,

which permit us to believe that illuminating gas is only composed of vapors. The experiments of Pitschke, cited in Schilling, are the only ones which establish the existence of naphthaline in gas. I shall discuss these experiments further on. It is easy to prove that the $C_{10}H_8$ which Pitschke has found, did not pre-exist, and that the nature of the conditions to which he submitted the gas had given birth to it for him. Analysis of gas shows the presence of ethylene, of butylene, of acetylene, of marsh gas, of the oxide of carbon, of carbonic acid, of hydrogen, of nitrogen, but *not* of naphthaline. Naphthaline does not pre-exist then in gas. It forms itself there and under certain influences. No more does benzine pre-exist in gas for it would dissolve the naphthaline in it, either that which the gas contained or that deposited in the pipes; analysis moreover does not show its existence in it. However, in the street mains we find naphthaline in dangerous proportions, and in the drips we detect the presence of benzine. Both are produced under the same circumstances and under the same influences, but with this difference always, that naphthaline can be produced from benzine, and that benzine cannot be produced from naphthaline. If I establish this parallelism between the two bodies, that is that the igneous reactions of which I have spoken above, make known the relative relations existing between them, and that it is thus that I happen to establish the production of naphthaline in the gas, and consequently that it did not pre-exist there. I will describe an interesting fact that I have observed, and that I think it necessary to make known.

Upon one of the main outlet pipes of my works, whose diameter is 350 millimeters (14 inches) I have had a branch at a distance of 1200 meters from the point of departure, a new condict of 300 millimeters in diameter, which is supplied only from a single point and at intermittent periods. In one place this pipe which was only .80 C. below the surface, descended, by necessity to a depth of four meters (five feet) and this for 700 meters nearly to its junction. The syphon that this difference of level necessitated was always filled with a blackish liquid, very rich in benzine, while the syphons at the outlet of the works, and of the main pipe which supplied this branch, gave only water with some traces of light oil. This tends to prove that the gas that sleeps, so to speak, in this pipe ot 300 millimeters, unused at certain times and sometimes for long periods, undergoes certain influences in which time also acts, gives birth to benzine, and that it did not pre-exist, since through all the course of the gas before the point, that is to say for a length of nearly two kilometers (5 miles), we find in the liquids of condensation only mere traces of benzine (C_6H_6). I have said that the experiments that I made established the fact that gas entirely dried, entirely deprived of aqueous vapor, could not deposit naphthaline. The following are the experiments:

1st. I took upon the same courant of gas two branches, one which conducted the gas in its ordinary state, in the other it passed successively through three flasks containing each one kilogram of chloride of calcium in such a way that the gas was entirely dried. When the first flask became wet I renewed the chloride of calcium that it contained. The gas from each of these two branches followed the same course in glass tubes, kept parallel, which were joined to a series of Liebig tubes, which were plunged into the same salt bath, kept constantly at a temperature of 75° Cent. Upon leaving the bath the gas circulated in parallel tubes and entered a new series of Liebig tubes, plunged in the same receiver filled with ice, and sometimes with a refrigerating mixture, at the outlet of which, after passing over an identical course I caused the dry gas to absorb water by simply flowing through it. The gas thus restored to state of ordinary moisture returned by similar tubes to the first, into the same salt bath which it passed through in like Liebig tubes. I reproduced in this way in an exaggerated form, what actually occurs in practice, the gas passing suddenly from a high temperature to a very low one, and changing velocity in consequence of the change in section of the pipe. It is useless to say that I operated upon gas perfectly purified, and that the quantities of gas with which I experimented were equal.

This experiment lasted for a month, and that to in the same month of December that the same gas produced an engorgement from naphthaline, in the outlet of the station meter of which I have spoken. That upon which I experimented was taken off before it arrived at this apparatus. Let us look at the results. I never obtained a trace of condensation or a trace of naphthaline in the tubes through which the dried gas passed. I did obtain condensation, and always naphthaline, not only in the tubes where the gas was not dried, but also in those tubes where the gas dried at first had given none, but had retaken moisture from the last flask.

I think this first experiment conclusive, and I add that the dried gas had gained in illuminating power, as was easy to forsee.

Second Experiment.—As I said before, naphthaline made its first appearance in my works, in a considerable quantity, upon the surface of the purifying material in the boxes, and upon the interior of the covers. One of these, in particular, permits the crystallization of naphthaline to take place in proportions such that we cannot think the naphthaline was produced far away, for it would all have been stopped there if this naphthaline had pre-existed in the gas. I divided this box in two parts. In one of these parts the oxide of iron was left as usual, in the other part I put a layer ot oxide of iron 20 centimeters thick, and then placed quicklime upon this for a heighth of 20 centimeters. At the end of several days I raised the cover of the box. The surface of the half that I had left in the ordinary condition (the oxide of iron part) was literally covered with naphthaline, and the corresponding part of the cover was spangled with it.

The part where I had placed the quicklime had not a trace of naphthaline, and the half of the cover over the quicklime had not a trace of it. The box was worked equally in other respects, that is to say, that below the 20 centimeters of lime, upon leaving the point where the whole box was filled with oxide of iron in the ordinary state, the material was blackened equally over the whole surface of the box, and at the same height. The lime was partially slacked by the water of the gas which it had absorbed. Thus, the surface of the material, and the cover of the box, showed the contrast of being upon one part entirely dry and free from naphthaline, while upon the other part naphthaline was perfectly sublimed in brilliant and moist particles.

A third experiment, which is a reproduction, upon a large scale of the first, is now in progress, but I cannot now make known the results, as I intend to make it last for several months.

I feel authorized to quote from a letter that I have received from one of my colleagues, the manager of a large works, in which the purification is by lime exclusively, not quicklime, but lime slaked in powder, and susceptible still of absorbing a considerable quantity of aqueous vapor. I quote literally from this letter, which was not written by request, the author being ignorant of my experiments, and the theory which flows from them. "I must tell you, first of all, that my condensing apparatus is sufficiently powerful, and that, consequently, deposits of naphthaline sufficient to produce an obstruction in the ordinary working are very rare. My purification is still entirely by lime. I have little or no naphthaline at the inlet or outlet of my purifiers, and it is only from sense of duty that I overhaul this part of my pipes once a year.

I have more of a deposit at the inlet of the station meter, and *above all at the outlet*."

I underline these last words, which confirm my statement, that naphthaline is produced at the outlet of an apparatus in which the gas is saturated with the vapor of water, and when it gives up this vapor by condensation. The influence of condensation of aqueous vapor contained in the gas upon the production of naphthaline is, then, evident and demonstrated by these experiments. This is the third cause, of which I spoke at the commencement of this memoir. It is only explained by the reproduction in the mass of the gas of reactions, analogous to the igneous reactions which I have described above briefly.

The gases of which illuminating gas is composed, under certain influences, notably those of heat, as found by M. Berthelot, are changed into other bodies, and by a series of inverse decompositions and reciprocal displacements, produce benzine, and from that naphthaline. If chemists can by the aid of appreciable forces, cause bodies to react upon each other, decompose them and form them again, these bodies, placed in favorable conditions, with smaller forces, can naturally, so to speak, cause the same reactions and produce the same new bodies that Science has created with means less powerful. Is not, moreover, the condensation of aqueous vapor a source of heat? Does not the vapor of water in condensing abandon all the heat that has been absorbed by the change of state from water into vapor, otherwise called the latent heat of vaporization? And cannot these quantities of heat thus restored, in the mass of gas, play the same role as the heat with which igneous reactions are produced? Would not the reactions which change marsh gas into ethylene, ethylene into acetylene, acetylene into benzine and styrolene, styrolene and acetylene into naphthaline, benzine and ethylene into anthracene and hydrogen, anthracene and ethylene into naphthaline and benzine, etc., etc., cause a disengagement of heat if they were produced; and the condensation either of benzine or naphthaline, would it not cause the giving up to them also the latent heat of vaporization of these two bodies? Is not the impact of the gas in the pipes also a source of heat, which, although inappreciable to us, could favor the reactions of which I speak.

I spoke of the experiment of Pitschke who found that "in causing gas to pass through a serpentine glass tube, about 9 metres long, filled with chloride of calcium, and cooled to 18°C., benzine and naphthaline were separated, the first especially in a quantity very appreciable," (the second $C_{10}H_8$ was not, it appears, in a very appreciable quantity) "then that in passing ether through the gas, and evaporating, he obtained benzine and naphthaline."

I am not surprised at this last fact, for ether dissolves the hydrocarbons, notably, ethylene; the benzine and naphthaline could thus have been produced either during the act of the dissolution of the hydrocarbons in the ether or during its vaporisation. The experiment could only be conclusive upon the condition that benzine and naphthaline were exclusively the only hydrocarbons which, existing in the gas, were soluble in ether. As to the first experiment, it would not be conclusive to me. In order to produce benzine and naphthaline "first, especially in quantity, very appreciable," it is necessary to pass a certain quantity of gas, and one can easily describe the difficulty of filling a serpentine glass tube, of which the diameters are generally very small, with the chloride of calcium. This would lead us to think that the gas which Pitschke examined had not been always thoroughly dried—the drying of the gas being a very delicate operation. If it had not been thoroughly dried, the condensation of aqueous vapor by the refrigerating to 18° has produced, as I have indicated, the naphthaline which was deposited. The same chemist produced nitro-benzine, by passing gas through fuming nitric acid. He does not say that he produced nitro-naphthaline, which he would easily have distinguished from the first by their different properties. The nitro-benzine solidifies at 3°, and nitro-naphthaline only at 43°.

Refrigeration alone does not cause deposits of naphthaline, and it is an error to think that the personal experience, which I have cited above, proves that cold produced naphthaline. It was only produced because the condensation of aqueous vapor is more abundant in winter than in summer; but as the gas is by the cold more easily deprived of its aqueous vapor after the condensation of it does not produce itself. I read in in Knapp's Chemical Technology:

"At all times we find cases where, in spite of refrigeration carried to —20°C, we cannot obtain any naphthaline."

Before closing, although the considerations which follow do not relate directly to the theory which I maintain, I think it is well to explain why the deposit of naphthaline is not always found at the point where the condensation of aqueous vapor takes place. The naphthaline is produced in this place, but it is not deposited there always, because, as the flakes are very light, and drawn along by the current of the gas, they are deposited where, by a change of velocity, an obstruction, a bend, the gas finds an eddy, or, so to speak, a time of arrest: or, where it meets a material obstacle, the rough sides of the pipe for example, then wherever it has been deposited by any one of these causes whatever, it forms an obstacle of itself to the naphthaline which the gas carries along, and holds in suspension, and little by little the deposit augments at this point, its flakes accumulate, one upon the other, upon the whole periphery of the pipe. Often, in house services and street lanterns, during severe winters, the water of condensation freezes, and the crystals thus formed constitute the material obstacles which stop the flakes of naphthaline which otherwise would be carried to the burners. To sum up. The production of $C_{10}H_8$ is due to the distillation of coal at high temperatures, but this is entirely dissolved by the contact of the gas with the tar and light oils which contain it. The production of naphthaline varies with different coals, and the constituent elements of the gas made differ with the coals also.

The more that a kind of coal contains, as elements constituting illuminating gas, of marsh gas, ethylene, acetylene, styrolene, and anthracene, the greater will be the proportion of naphthaline that this kind of coal will produce. The production of naphthaline, after leaving the purifiers, is due to reactions which I shall call intimate, which take place (produce themselves) in the different elements that constitute the gas; the condensation of aqueous vapor causing these reactions—or, at least, these reactions do not take place, or, rather, naphthaline is not deposited when the gas is entirely deprived of aqueous vapor. This being admitted I have only to describe the means that I have employed to accomplish the desiccation of the gas, which is too simple to require a long description. This process, the application of the above theory, I have patented.

At the outlet of the purifiers, as they are already established, the gas passes through one one or more boxes filled with an agent that has a great affinity for water, quicklime, potash, soda, baryta, etc., etc; the hydraulic joint, if the seal is thus made, is produced by means of any non-essential oil. The syphons are filled with oil; the station meter is filled with oil; in fine, upon the water of the gasometers is a bed of oil some centimeters in thickness. After passing the outlet governor, for more safety, the gas passes through some columns or boxes filled with the agent having an affinity for water, adopted in the first apparatus. The street syphons are filled with oil like those at the works.

A serious objection arises here. The consumers' meters would rapidly have the water line lowered by the gas which arrived dry, and, consequently, with great affinity (avide) for water.

It will be necessary to use others, or to adjust their level oftener. This, at any rate, would be an inconvenience very trifling compared with those resulting from the deposit of naphthaline, which, at certain times, either about the works or in the canalization of the city, are of so grave a nature that they might compromise the services of manufacture and of the public lighting, and also cost the gas companies considerable sums—sometimes entirely a pure loss. I shall be happy to have found, if not the first cause of this evil and the remedy for preventing it, at least one of the causes, and to have demonstrated and indicated a simple proceeding which allows to remain in the gas the elements which, at a given moment, produce naphthaline; but which (not producing it) contribute to the illuminating power of the gas, and are sold, in place of being a nuisance and a loss. This evil is thus changed into a triple source of benefit; the expenses caused by searching for and the labor for combatting the obstructions become useless; gain in illuminating power; increase in the sale of gas by a volume corresponding to the quantity of naphthaline crystallized and lost.

Thirdly. The results obtained, and the means used for the prevention of the deposit of $C_{10}H_8$, are as various as the localities where it occurs. They are all based upon the solubility of $C_{10}H_8$ in tar, or in the application of some of the tar products which are solvents for it, to those parts of the works where it is supposed to form. By prolonged contact with tar, as by the friction, scrubbers, etc., good results have been obtained, though these even cannot overcome more than a certain amount, as there is a limit to the absorbing power of the tar.

In some works an immediate use of large quantities of enricher is resorted to as a cure, and, generally, with success for the time being. Sometimes this enricher is in the form of cannel; sometimes some form of petroleum put into the retorts, with the caking coal, removes the trouble, and then, again, these are tried and fail. I have been told that, in the manufacture of gas by the Tessie du Motay process, where from five to six gallons of naphtha used per 1000 feet of gas produced, instances have occurred where naphthaline had formed and completely choked a six or eight inch pipe in the works itself; and, from the reactions spoken of by M. Bremond, may be found an explanation of it.

Another method for preventing it is sprinkling benzine upon the purifying material after the boxes are filled. This, it is claimed, is efficient; but I have known of instances where it did no good You have all heard the paper, by our friend, Mr. Curley, of Wilmington, Del., which gives you the facts in his case, and his system would seem to commend itself to your most favorable consideration; but if the naphthaline forms itself spontaneously at any point of the manufacture after the scrubber, and also in the distribution when the proper conditions therefor are presented, we shall still have trouble. If M. Bremond is right and his experiments, which I have read, are vouched for by well-known and reliable witnesses, it would seem that in his method was to be found great aid in freeing you all from this troublesome $C_{10}H_8$, viz.: To deprive your gas of the watery vapor which it contains and keep it so till it reaches the burner. The means he used to prevent the retaking of aqueous vapor was by using oil, in his purifying seals, his station meters, his syphons, and on the surface of water in the holder-tanks.

A more simple method might be to have a box of quicklime for the gas to pass through, after leaving the valve-house, just as it enters the mains for distribution.

With the means in use for freeing lamps from wood alcohol, naphtha, etc., you are all familiar.

Thanking you all for the careful attention you have accorded to this already *too long* paper, I close with the hope that it may the humble means of directing your minds into channels of investigation that shall prove of permanent value to the profession which you represent.

When Capt. Dresser concluded he said: I wish to apologize to you, gentlemen, for keeping you so long. I tried to cut this down, but the more I tried the less able I was to do it, and I finally thought it would be better for you to have it just as it was.

Mr. Forstall—I don't think Capt. Dresser need apologize at all, for I am sure we have all been very much entertained.

The President stated that Mr. Goodwin had an apparatus for testing gas, which he would be glad to explain to the Association; and upon motion it was decided to request Mr. Goodwin to make an analysis of gas with his apparatus during the evening session' provided there was sufficient time left after the discussion on the various papers yet to be read.

The Convention then adjourned until 7 o'clock, P.M.

EVENING SESSION.

The members of the Association convened at 7.30 P. M., and were called to order by the President, who said: Gentlemen—When we took our recess we were on the subject of naphthaline, and the meeting is now open for discussion, remarks, and questions. I am very glad the subject has been presented to us, and very glad we have had two or three papers read. Naphthaline is a great nuisance, and is getting to be more and more so. It makes considerable expense to the company by requiring a large force, who have to be paid for their work: but that is a matter of no great consequence—but when it annoys our consumers to the extent that it has done, putting out lights, in a store or church, for instance; or, when a person tries to light the gas in his house and finds no gas, and he may be a long distance from the office, and it may be very difficult for him to find any one—therefore, I say, I feel the annoyance resulting from naphthaline to our consumers more than to ourselves; and if we can find any radical cure for this naphthaline, then all the expense and trouble we have incurred by coming here will be paid back ten-fold.

Mr. Wood (of Syracuse)—I would like to know how general this complaint is? Over the central part of the State of New York, from the Hudson River to Buffalo, we hardly know what it is. We occasionally have a little in the service pipes, but very little; and I am informed that it is so in Albany. We use the best of coal, and we are not troubled, to any extent, with it. I know there is a good deal of time spent at every meeting on this question of naphthaline, but to me it seems a waste of time.

The President—I have met gentlemen, and, in speaking to them on the subject, they have said "Why, I don't know what naphthaline is." Six months afterward I have met them again and asked them if they knew anything about it, and they have said—"Oh! Don't mention it; we know well enough now what it is." So, Mr. Wood, you are liable to be attacked at any time.

Mr. McIlhenny—I don't know that I can throw one ray of light on this mysterious subject; but I would like to dispel what little mystery there is between the papers which have been read. Mr. Curley proposes to vaporize the naphthaline in the retorts and carry it along so that it will not be deposited. In the paper read by Captain Dresser it was proposed to remove the aqueous vapors by dry lime used on the purifiers.

Captain Dresser—Not lime, but chloride of calcium, or something of that kind, on the purifier and subsequent box.

Mr. McIlhenny—I didn't try this experiment; but I want some simple, practical way, and I think it could be used on one, two, or three trays of the purifier. That is one method of preventing it, and the other is to fill the meter with benzine.

Captain Dresser—Any non-essential oil. The object is to prevent the contact of the gas with the water after it has been dried.

Mr. McIlhenny—My object is to put it, so that it can be distinctly understood, how to do it. There are, then, two ways—one the use of quicklime in the purifier, and the other, and most convenient and simple, is to use any light oils to flow on the top of the water and prevent the gas from absorbing moisture.

Captain Dresser—it is not two methods—they are used together. The object of the first is to dry your gas, and the object of the second to keep it dry. It is all one process.

Mr McIlhenny—Well, I simply wanted to reduce it so that any one here could try it, if necessary. All you have to do is to use the lime on a part or the whole of your purifier, and then use the oil in the manner I have stated.

The President—Suppose you use the lime on every tray of your purifier What is the result?

Mr. McIlhenny—I don't know; I never tried it. Well, you use lime just the same, but it is a different lime.

Mr. McElroy—On seeing this process explained in the GAS-LIGHT JOURNAL, I determined to give it a trial, and the only way I had to do it was to fill one tray with quicklime. I thought I would have an opportunity to experiment before coming to the meeting, but, unfortunately, the box held out longer than I expected, and I had no chance to see the effect, consequently, I cannot give it to you. Naphthaline has been a great annoyance to me, and one of the worst things about is that you don't know where or when it is going to strike you. It is like the measles—you all have to have it sometime. Why, at one time, I had 270 complaints out of 5,500 consumers, and I certainly think that anything that can be projected here to cure this annoyance will amply repay us for coming here. I have tried everything. I have covered my purifiers with lime, and sometimes I would succeed, and at other times it wasn't worth shucks. [Laughter]. I got up an instrument for vaporizing benzine, without bringing it into contact with steam, and, during the greatest consumption in the evening, I would pass that right into the main with the gas, and that, heretofore, until this year, had the desired effect. I could always pill the monster in two nights, but this year it got me down, and got stronger every night. [Laughter]. But the morning I left home to come here I only had eighty calls, so it is getting better. I was in hope I could try this desiccation process. I have a tar pipe in which all the tar and drip from the retorts pass. This is an inch pipe. I have a three-quarter gas pipe, with steam at about 80 or 90 pounds pressure, and you can't hold your hand on that pipe, so that, if there was anything in that, I think I could drive it off. It has been in operation for six months every day and night, and it don't reach the trouble, so I don't give much for that. [Laughter.]

Mr. Lindsley—I feel a good deal interested in this subject. For three years I was ignorant of naphthaline, but learned to my sorrow afterwards what it was to the extent of having from four to six inch pipes stopped up with it at various times. For the latter part of that time I would use the test gauge to discover it, get behind it, apply fire so the heat would go forward in the direction of the gas, and, in that way, I have thawed it out; but Mr. McElroy has occasioned me a momentary pang. [Laughter]. I have labored for some little time under the delusion that I had a good thing, and I thought I would communicate it. I will not give any scientific plan but relate my experience. I have sprinkled my purifiers with benzine for two years, and had a steamed gas main; and, in addition to that, I have had a pan, six feet long and three feet across, near the inlet, and for the last two years I have had but very little trouble, so that I did think naphtnaline didn't amount to anything after all. Once in a while it indicated its presence, but I would send the boys around and dispel it, so that, for two years, I have had a good time; but Mr. McElroy has frightened me considerably. [Laughter.]

Mr. McElroy—I didn't include light tars in my heater, so there is some chance for that yet. I have dissolved naphthaline with steam, and, at first, I thought it was a bully thing [laughter]; but afterwards I found I could not get anything to wash it out. Put benzine in, and boiled it; the result was I had afterwards to pump out the boiled benzine. In short, I find when I keep my yield up to five, or 5·20, I can't cure it, unless this desiccation process will cure it; but if I reduce it down to 4.95 then I can handle it with benzine. Above that—I can't control it.

Mr. Coggshall—What was your production, Mr. Wood?

Mr. Wood—Our average yield for 12 months is five feet to the pound.

The President—I have used cannel coal and had naphthaline; oil—naphthaline; Penn coal—naphthaline; and several years ago I was making gas with iron retorts, less than three feet to the pound, and had naphthaline.

Mr. Rankin—I would ask Mr. McElroy if any vapors are engendered at the same time with the gas?

Mr. McElroy—Water of distillation is certainly present as it comes from the retort house.

Mr. Curley—I can easily understand why that is from the fact that he gets such a large yield. If I could run off 5 feet naphthaline would make its appearance. My production, per ton of coal, is 10,800 feet of 17 candle gas. If I run up to a larger quantity of gas I deprive it of all light vapors. When they are decomposed they add very little illuminating power to the gas; they increase the volume somewhat, but very little; but I find that a small quantity of vapor mixed with the gas make it six or ten times more valuable, as illuminators, than the same vapor decomposed in the form of gas.

Captain White—I will supplement Mr. Curley's remarks with a little of my experience, which I will leave as a legacy to the Association. My gas averaged nearly seventeen candles for two years, with a yield of 11,000 feet to the ton of coal, and I used nothing but West Virginia coal—no enriching material. The method used was something similar to Mr. Curley's method. The process was simply to first use a friction scrubber—a very simple affair, consisting of a series of boxes with perforated screens—a series of screens. I carried all the regular products from the hydraulic main into these boxes. The gas left the retorts under the action of the exhauster, and passed through these boxes, passing through the screens and so breaking up the molecules, and the vapors were received by the gas and mingled with it as it went along. The process was simply that and nothing more. It went on through the ordinary process, the purifier, etc., and we never knew what naphthaline was, and never had any trouble about the illuminating power. The gas was always over 16 candles. Another gentleman, who had no interest whatever in the process, tried it with the same results that I obtained. I concluded when I was going to leave the company, for certain reasons, to take that thing out. I did so, supposing my successor would take charge of the works this month; but he did not come, and—note the result. In two nights after I had taken out that scrubber, I had 100 people in the office calling for gas. They had no light. A few days ago I got mad; went out and purchased some coal oil, mixed it with sawdust, and stopped it up, and as long as I kept my production at five feet, or a little less, I had no trouble whatever.

It is all very well to pass it through dry lime, and grease your mains, and all that kind of thing; but if you go to work and put that scrubber in your works (and it won't cost you more than $100) you have a solution of the whole thing. And it is a gain to your companies too. We have not had to purchase any high-priced coals, and have saved all the labor of going around and cleaning out the service pipes. A few thousand dollars look better on the dividend account than on the expense account, and there is where I managed to keep it while I had that scrubber.

[To be continued.]

Projected Railroads in Japan.

Mr. R. V. Boyle, Engineer-in-chief of the Japanese Government Railways, has recently made an examination of certain lines believed to be most needed, and reported thereon, dividing the proposed lines into sections, of which he speaks separately. The first section is from Tokio on the Gulf of Yedo northwest to Takasaki, 66 miles passing through Inatsuki, Omiya, Tuyaka and Okabe. Much of the work on this section would be light, through some large river crossings are needed over Toda-gawa, and protection of the road-bed against floods in that river would also be necessary. Takasaki is town of much importance, with a considerable trade, and there is already much traffic along the proposed line, which could probably be much increase by the railroad.

The second section is from Takasaki nearly due west to Matsumoto 3 miles, and this section will require some heavy work in the hills about Shinani and a tunnel about a mile and a half long at the head of the Shiroya Valley. There is much broken and mountainous country along the line, requiring the heaviest grades and sharpest curves the rood, besides, for most expensive work. But little local traffic can be looked for, but this section would be of value as a part of the through line and as connecting populous provinces now separated by a mountainous district crossed by few roads, and those very poor.

At Matsumoto the line turns sharply to the southward and runs in that direction 70 miles to Nakatsugawa-Yeki. The work on this section will be lighter than on the previous one, though some heavy grading will be needed on the divide between the Sai-Kawa and Kiso-Kawa valleys. For the most part, however, the line follows a natural and comparatively easy route through the river valley, needing some bridging, but none of very large size. Much local traffic would come to the line, besides a considerable busines in lumber, which is floated down the mountain stream.

From Nakatsugawa the road will turn westward, and run in that direction fifty-five miles to Kano and Gifu, following the valleys of the Toki-gawa and Nemoto-gawa, and then crossing a low range of hills, down the Oahri-gawa and Kiso-gawa to Gifu. The grades on this division will be light, and the only expensive bridge needed will be that over the Kisogawa, near Doda.

These four sections complete a main line of 271 miles from Tokio to Gifu, and from the latter place a line already surveyed runs south-west seventy-four miles to Kioto, making a total distance of 345 miles from Tokio to Kioto. At the western end the line would connect with the 49 miles of road now in operation from Kioto to Kobe, and at the eastern with the 18 miles from Tokio to Yokohama. The line marked out connects two principal cities, and follows an already established route of travel and trade, which is believed to be capable of great development.

The last section surveyed forms a branch of this main line from Uyeda and Tanaki north by east to the important sea port of Niigata. This branch will require only a moderate amount of heavy work, though much of it is through a broken and hilly region, and a good many bridges will be needed. There will be little rock work, and most of that is through a light and easily-worked rock. For much of the distance the road follows the Chikuma-gawa, and it will have to meet the competition of boat navigation on that river for nearly 75 miles. Much of the line, however, is through a very rich rice country, from which a large traffic can be carried on to the main line.—*Iron.*

Prices of Gas Coal.

The market presents no new feature. Prices of the domestic coal remain without change, except so far as New England deliveries are affected by the enhanced water freight from Baltimore. These latter now range from $1.75 @ $2 for ports east of the Cape—making the Penn and Westmoreland coal cost from $6.25 @ $6.50, according to the port of delivery. When Provincial and English coal, of approved quality, can be delivered at the same ports at $4.00 @ 4.25, it is not surprising that the sales of American coal have been materially decreased from any recent previous year, and the importation of foreign coal largely increased. A fair estimate of the annual requirements of New England gas companies, east of Connecticut, will show not far from 225,000 tons. Over two-thirds of this quantity is delivered at points accessible by vessels of large size, and, therefore, open to foreign competition. With a margin of over $2.00 per ton in favor of foreign coal, it has not been difficult for the Provincial and English shippers to regain a large share of the trade which they formerly enjoyed in New England. That they will hold it, while the present combination between the Pennsylvania Railroad and Baltimore and Ohio Railroad Companies continues, cannot be doubted. Gas companies feel the necessity of economizing as well as other manufacturers,

and their pecuniary interest demands the securing of cheap coal—quality being a primary consideration. If the cost of our domestic coal is relatively higher than that of the imported article, resort will be had to the latter to a great extent; and as the item of railroad transportation regulates the cost of the home product, the sooner that is adjusted to meet the demands of the time, the better it will be for the producer, the railroad company, and the consumer.

Among the importations of the last fortnight we note a cargo of 1200 tons Ince Hall gas cannel, to fill an order gsven some little time since; cost not transpired, but presumed about $8.75 delivered.

Nominal quotations of the standard gas coals continue, viz.—

Westmoreland	$5.50	$4.50 } at Baltimore.
Penn	5.50	4.50 }
Despard	5.35	
Marion	5.35	
Murphy Run	5.35	
Newburgh Orrel	5.35	
Ches. and Ohio RR. Caking Coal	5.40	($4.10 f. o. b. at Richmond.
Block House	4.50	1.75 } Gold f. o. b. at Mines
Glace Bay	4.25	1.60 }
International	4.25	1.60 }
Clover Hill	4.00	
Cannelton Cannel	10 00	
Peytona	10.00	

Gas Stocks.

Quotations by G. W. Close Jr., with W. B. Scott & Co., Broker and Dealer in Gas Stocks.

24 Pine Street, New York City.

November 16, 1877.

☞ All communications will receive particular attention

Gas. Co.'s of N. Y. City.

	Capital.	Par.	Bid.	Asked.
Central, Westchester	466,000	50	90	95
Harlem	$1,850,000	50	97	100
Manhattan	4,000,000	50	210	—
Metropolitan	2,500,000	100	131	135
" Scrip	$1,000,000	...	100	103
" Bonds	500,000	1000	92½	95
Mutual	5,000,000	100	90	95
" Bonds, gold.	900,000	1000	105	—
Municipal	1,000,000	100	70	80
New York	4,000,000	100	123	126
Suburban, W'steh'str	390,000	50	—	100
Gas Co.'s of Brooklyn.				
Brooklyn	2,000,000	25	170	175
Citizens	1,200,000	20	70	75
" Scrip	320,000	1000	95	97½
Peoples	1,000,000	10	—	40
" Bonds	$ 325,000	...	—	95
" Scrip	300,000	...	75	80
Metropolitan	1,000,000	100	65	70
Nassau	1,000,000	25	—	78
" Ctfs.	700,000	1000	96	98
Williamsburgh	1,000,000	50	120	125
" Scrip		...	100	103
Union Co. E. N. Y.		25	—	50
Richmond Co., S. I.	300,000	...	80	90
Out of Town Gas Companies.				
Bath, Maine	70,000	100		
Buffalo Mutual, N. Y	750,000	100	65	70
" Bonds	200,000	1000	95	100
Baltimore, Md	2,000,000	100	134	134
" Ctfs., gold	1,000,000		106	110
Bayonne, N. J		100		90
Brockport, N. Y	25,000	100	...	80
Citizens, Newark	918,000	50	100	105
" " Bds.	124,000	—	—	—
Cincinnati G & C Co				190
Derby of Conn	160,000	100	60	80
East Boston, Mass		25	132	—
Fort Wayne, Ind		100	...	—
Hannibal, Mo	100,000	100	95	100
Hartford, Conn	700,000	25	184	186
Hempstead, L. I.	25,000	100	...	—
Jersey City	386,000	20	155	160
Jamaica, L. I.	25,000	100	...	—
Jacksonville, Ill	120,000	50	82	...
Lewistown Maine	400,000	100	50	85
Lima, Ohi,o	60,000	100	—	...
Bonds	30,000		90	
Laclede, St Louis Mo.	1,200,000	100	100	110
Long Branch		20	40	50
Peoples, Jersey City		...	120	125
Peoples of Albany	650,000	100	-	27
" Bonds	350,000	1000	--	-
Peoples of Baltimore		25	—	56
" Bonds			106	---
Perth Amboy		25	—	95
Rochester, N. Y.		100	50	60
Woonsocket, R. I.	150,000	100	100	...
Halifax N. S.	400,000	40	148	150
Hamilton, Ontario	150,000	40	117½	
San Francisco Gas-Co., S. Fr'isco Cal.			111	112
St. Louis, Missouri	600,000	50	73	75
Stillwater, Minn	50,000	50	—	26
Saugerties, N. Y.	15,000	100	95	100
Troy, Citizens	600,000	100	—	60
Toledo, Ohio			95	97½

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CARBONACEOUS INCRUSTATIONS

FROM

CLAY RETORTS,

By GEORGE W. EDGE, Engineer of Jersey City Gas Light Company.

Having been fully tested by many Gas Companies in the United States and England, this method is submitted to Gas Managers with the fullest confidence that it will accomplish the desired result at the smallest possible expense and inconvenience.

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Charleston Gas-Light Co., Charleston.
St. Louis Gas-Light Co., St. Louis.
New Orleans Gas-Light Co., New Orleans, La.

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426--1ylam

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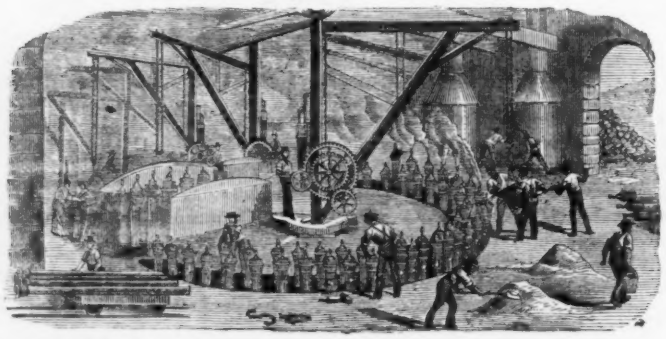

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ALSO ALL SIZES OF

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BRANCH CASTINGS, LAMP POSTS, Etc.

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HEAVY AND LIGHT CASTINGS OF ALL KINDS.

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N. B.—Pipe from 3 inch and upwards, cast in 12-ft. lengths.
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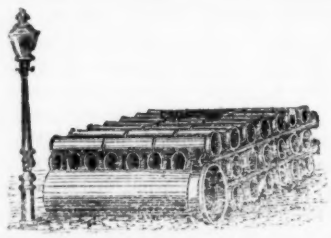

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It yields 10,996 cubic feet of gas to the ton of 2,240 lbs. of good illuminating power, and of remarkable purity; one bushel of lime purifying 6,792 cubic feet, with a large amount of coke of good quality.

It has been for many years very extensively used by various Gas Companies in the United States, and we beg to refer to the Manhattan, Metropolitan, and New York Gas Light Companies of New York; the Brooklyn and Citizen's Gas Light Companies of Brooklyn, N. Y.; the Baltimore Gas Light Company of Baltimore, Md., and the Providence Gas Light Company, Providence, R. I.

Best dry coals shipped from Locust Point, wharves, and prompt attention given to orders for chartering of vessels.
224-1y n

THE DESPARD COAL COMPANY
OFFER THEIR SUPERIOR
DESPARD COAL

To Gas Light Companies throughout the country.

Agents, PARMELEE BROTHERS, No. 32 Pine street, N. Y.
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Among the consumers of Despard Coal, we name: Manhattan Gas Light Company, New York; Metropolitan Gas Light Company, New York; Jersey City Gas Light Company, N. J.; Washington Gas Light Company; Portland Gas Light Company, Maine

*** Reference to them is requested. 204-

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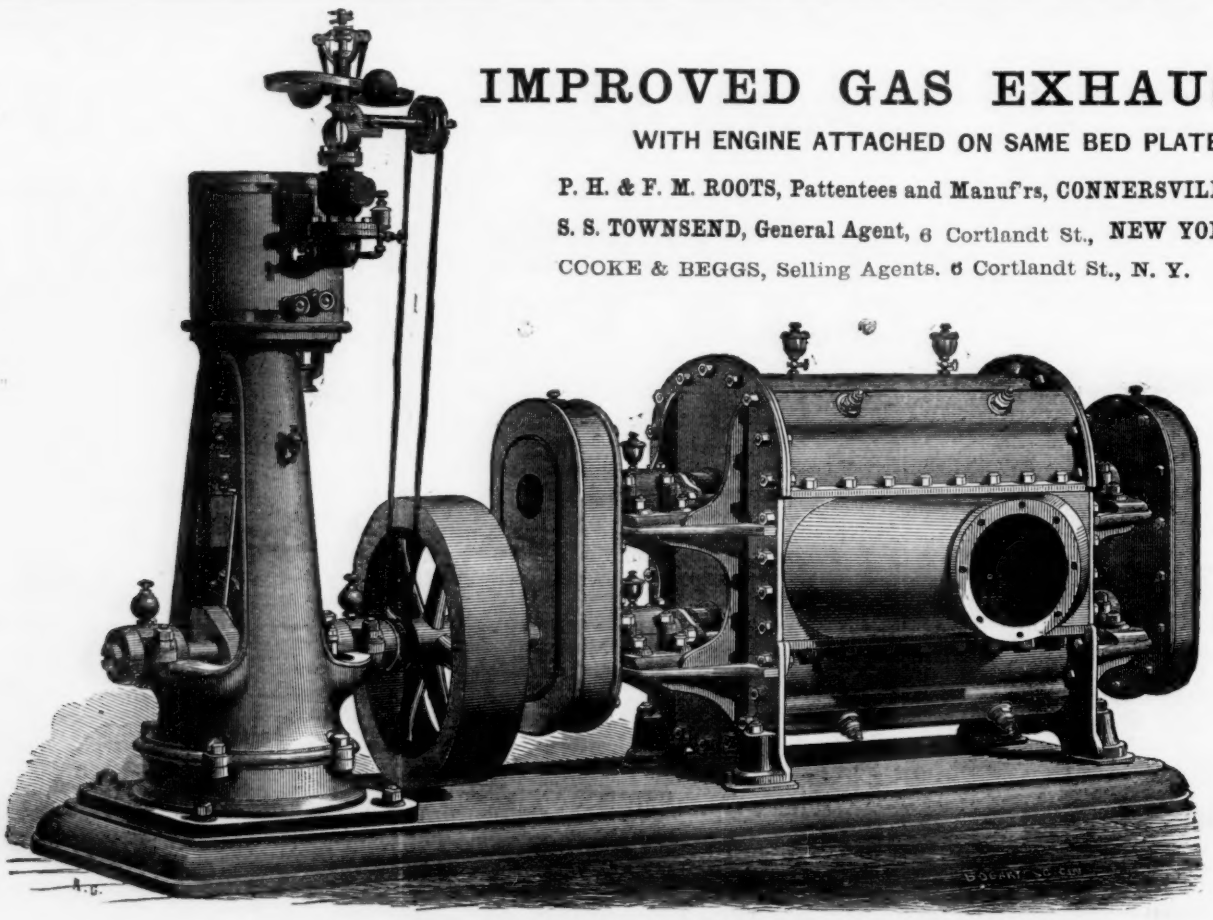

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PATENTED MAY 23rd, 1876.

THIS IS THE ONLY HAND MACHINE IN THE WORLD FOR CUTTING

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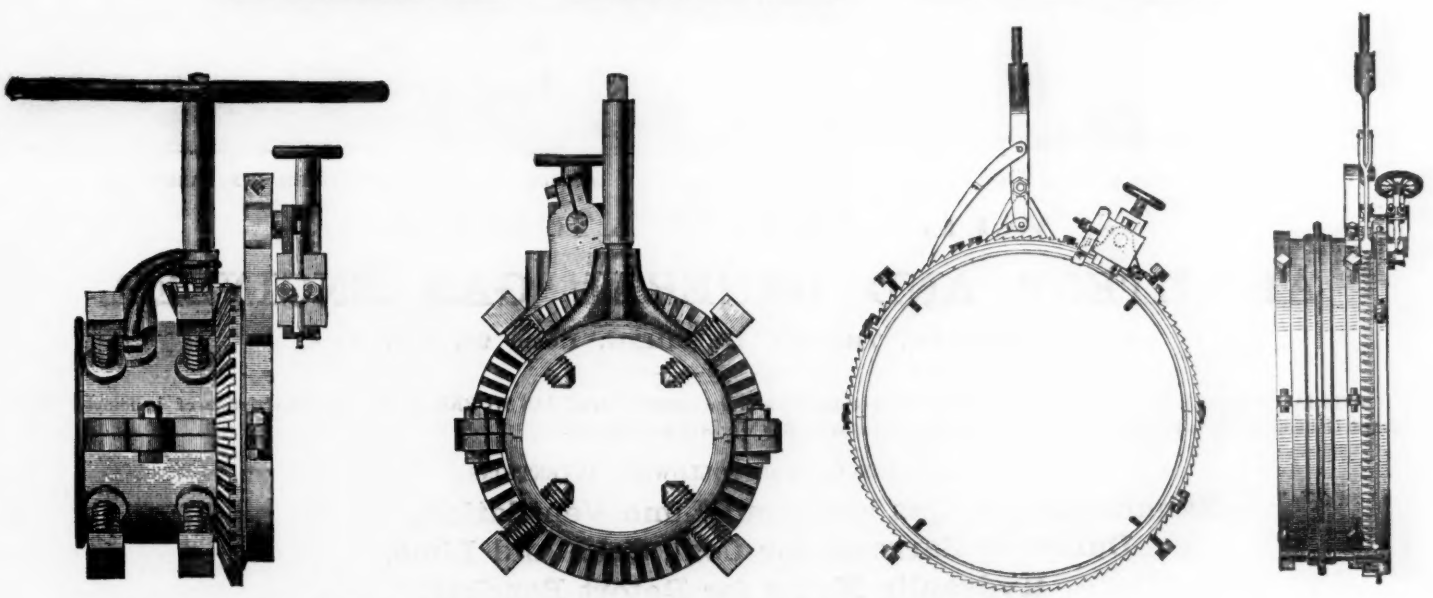

It will cut Cast or Wrought Iron Pipe, Shafts, or Columns of any size, making a clean, smooth, and square cut. Also,

A Continuous Line of Pipe in a Trench or Building.

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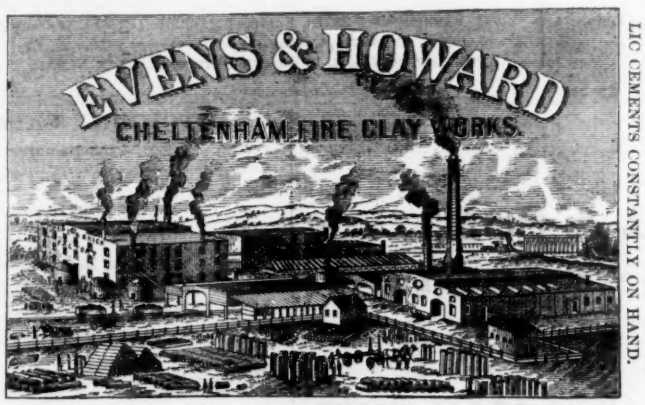

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300 Cubic Feet Capacity

WITH

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Where full particulars, as regards price, etc., may be obtained.

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Corner of Jay and Water Streets, BROOKLYN, N. Y.

HAVING GRANTED THE USE OF ONE OF my Patents to the Gas-Light Company of America and settled my claim against them, I inform the profession that I continue to erect Works and sell Licenses to use my Patents for making Gas from Petroleum.

You can enrich your Coal Gas to any desired standard, by using my process, in the simplest manner, and much cheaper than by *any other* method.

397 W. C. WREN.

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S. A. STEVENS & CO,

SOLE AGENTS.

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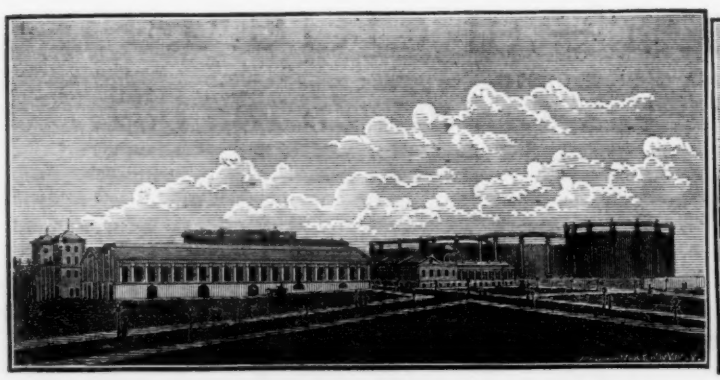
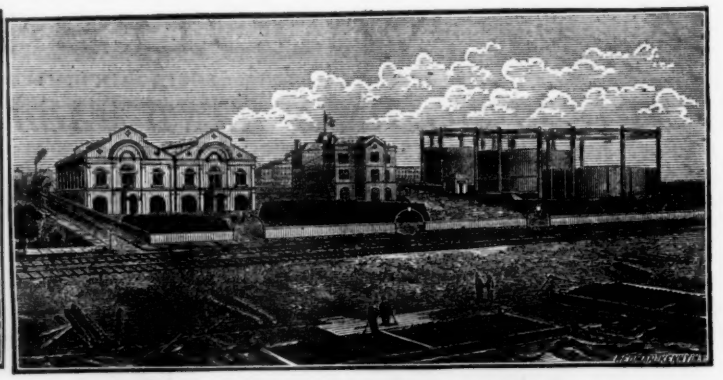


CINCINNATI GAS WORKS. ERECTED 1871-72-73.—WM. FARMER, ENG.



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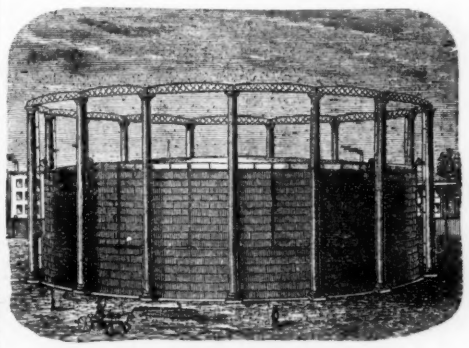

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Attest—J. L. CAMPBELL, Secretary, pro-tem.

Signed—A. T. GOSHORN, Director General

J. R. HAWLEY, President.

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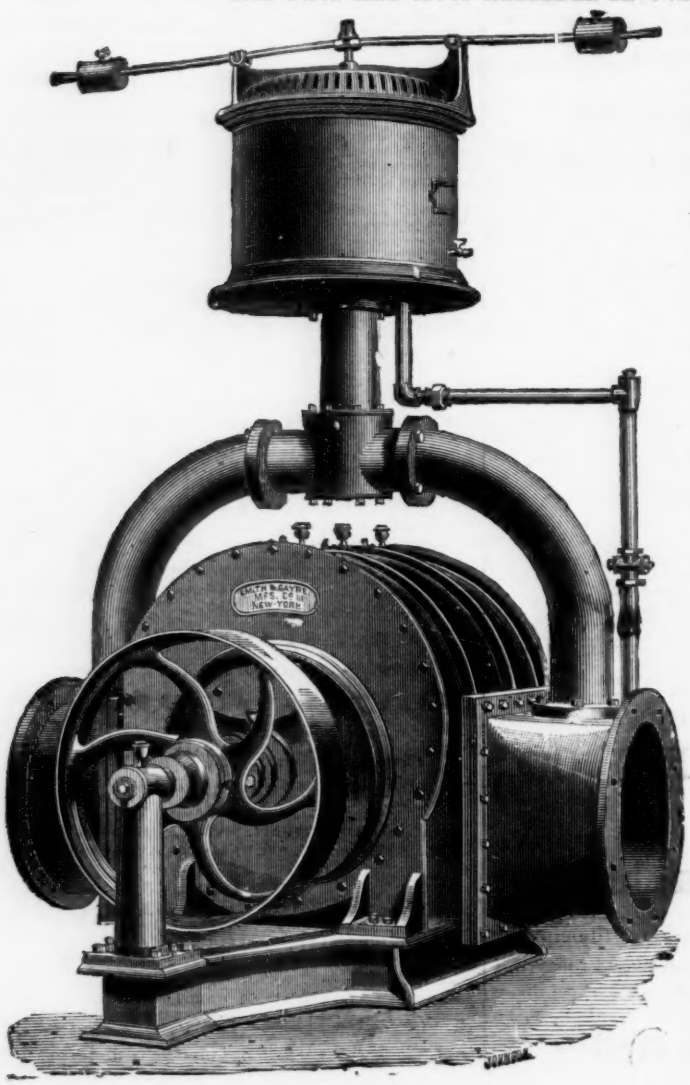

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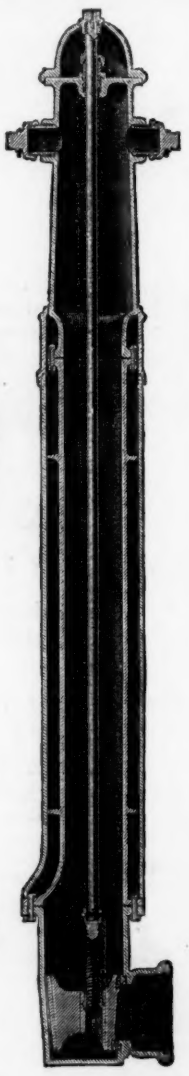

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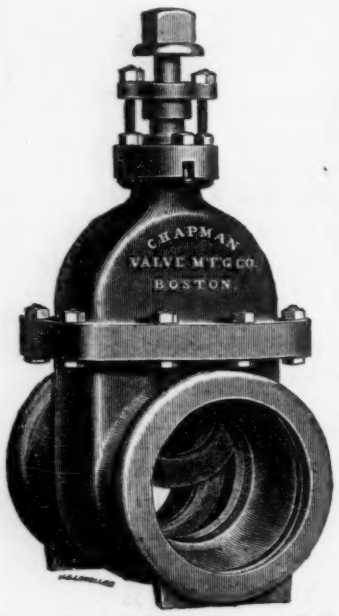

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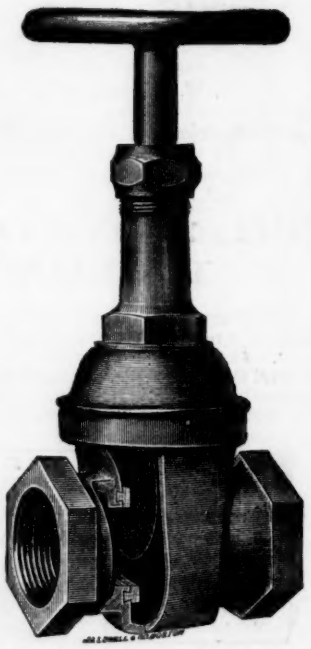

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Jamaica Plain Gas-Light Company,	"	Marblehead Gas-Light Company,	"
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Waltham Gas-Light Company,	"	Malden & Melrose Gas-Light Company,	"
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		Newport Gas-Light Company,	"
		New Haven Gas-Light Company,	Conn.
		Portland Gas-Light Company,	Maine.
		Lewiston Gas-Light Company,	"
		Calais Gas-Light Company,	"
		Rockland Gas-Light Company,	"
		Louisville Gas-Light Company,	Louisville, Ky.
		Cincinnati Gas-Light Company,	Cincinnati, O.
		People's Gas-Light Company,	Brooklyn, N.Y.
		Citizen's Gas-Light Company,	" "
		Williamsburg Gas-Light Co.	" "
		Brooklyn Gas-Light Company,	"
		Citizens' Gas-Light Company,	Newark, N.J.
		Peoria Gas-Light Company,	Peoria, Ill.

Office and Salesroom, 77 KILBY STREET, BOSTON. Works at INDIAN ORCHARD, MASS.